End of cold fusion in sight

Although the evidence now accumulating does not prove that that original observations of cold fusion were mistaken, there seems no doubt that cold fusion will never be a commercial source of energy.

IT seems the time has come to dismiss cold fusion as an illusion of the past four months or so. At the outset, on 23 February, the suggestion that deuterium nuclei can be made to fuse together at ordinary temperatures, if in exceptional circumstances, seemed a brave leap of the imagination. The article on page 29 of this issue by M. Gai *et al.* of Yale University is merely another nail in the coffin of the idea. The Yale group has done its best to replicate the conditions of the original experiments, but has failed to replicate their results. Similar outcomes have been reported from other laboratories. So what has been learned from these hectic months?

First, the negative results now being reported do not imply that the original observations by Stanley Pons and Martin Fleischmann at the University of Utah and by Stephen E. Jones and his colleagues at the Brigham Young University were grossly mistaken. Events may yet show that there are circumstances in which palladium electrodes in electrolytic cells emit pulses of neutrons just as they would if deuterium nuclei were fusing together; early this month, a group at the Los Alamos National Laboratory was wondering what to make of such an observation.

So far, all that is clear is that the original reports do not conceal a recipe for making large (Utah) or even modest (Brigham Young) amounts of power by deuterium fusion. For the many non-scientists who have been excited by the past few weeks, this will be a disappointment. By the same test, the managers of orthodox experiments intended to replicate what happens within the Sun will be relieved.

One striking feature of these events is that, even now, those who have been trying to replicate the original findings are remarkably good-humoured about the time and energy they have spent. Those concerned seem to have found it an inherently interesting exercise. It is not, after all, every day that they find themselves worrying about electrochemistry and nuclear physics at the same time. And it is interesting to have been reminded at first hand of the remarkable capacity of palladium and titanium to dissolve hydrogen, usually known only from books and journals. Moreover, the brief spell in April when it seemed as if cold fusion would permanently divide chemists and physicists has left no trace.

All of us, even bystanders, have also learned a great deal about the difficulty of counting absolute numbers of neutrons and of γ-rays. The argument between Richard Petrasso and his colleagues at MIT on the one hand and Pons and Fleishmann on the other about the γ-ray measurements have been for many people educative, to say the least. Petrasso and his colleagues (*Nature* **339**, 183; 1989) first complained of inconsistencies in the only published report by Pons and Fleischmann of their observations , were given an incomplete reply (*Nature* **339**, 667; 1989), but on that basis were able to argue (*Nature* **339**, 667; 1989) that the energy channels in the original equipment had probably been miscalibrated and that the energy spectrum is narrower than the resolution of the γ-ray detector would allow.

There is even doubt about the placing of the γ line purportedly resulting from neutron emission which has been variously reported as at 2.2 MeV and at 2.5 MeV. Pons and Fleischmann originally put it at 2.2 MeV, which is what would be expected if the γ-rays come from the conversion of neutrons in water. But now, in their reply to Petrasso, they say they could not have measured such a peak at such an energy, but that it is in any case at 2.5 MeV (which Petrasso disputes on calibration grounds). The best resolution of this dispute would be by independent measurement, but that seems unlikely while attempts to replicate the phenomenon as a whole are unsuccessful. Meanwhile, there will be many who consider the γ-ray signal to have been an artefact.

That point is nevertheless crucial to the unfolding of events after 23 February, when both *The Wall Street Journal* and *The Financial Times* published long accounts of what had been done at Utah and when the University of Utah held a press conference to tell the wider world. (*Nature* owes Pons and Fleischmann an apology for having reported that, on that occasion, they had said that their formal paper had been sent to this journal for publication.)

It is unthinkable that reports of the production of excess heat in such complicated electrochemical cells would, by itself, have been seriously regarded as proof of deuterium fusion. Only the measurement of nuclear particles and products, with the expected energy, could have commanded the interest since shown. Pons and Fleischmann now say that "as we have repeatedly pointed out, we are well aware of the deficiencies of these spectra", but there are no records of that reservation earlier than the meeting of the US Electrochemical Society at Los Angeles, by which time they had been sent (but may not have read) Petrasso's first draft of his complaint. This is a more serious retreat than they acknowledge.

None of this implies that Pons and Fleischmann have been anything but straightforward. Put yourself in their position if you believe otherwise. If, on 23 February, you had made such an arresting announcement that the whole world was agog, even picking up the telephone would probably engage you in a half-hour conversation with somebody you had never met. Your compelling interest, to gather more data, would be compromised by the inquisitiveness of people asking elementary questions about issues then, in your mind, settled. It is remarkable that Pons and Fleischmann, with all the pressure on them, should have been able to cover so much ground.

So how should they, in the contemporary argot, have played it? It is too easy to say that they should never have given the story of their doings to the financial newspapers, or have allowed a press conference to be held on their behalf. The conventional wisdom, that they should have sent an account of their work to a respectable science journal and then have put themselves in the hands of its referees and editors, is too bland. If people believe they have found a way of changing the world, why should they not tell the world what is in store in their own way?

But there are obvious dangers in such a course, of which the chief is that one may be mistaken. Ordinarily, there is no shame in that: people make mistakes all the time. Ordinarily, there are also colleagues to point to pitfalls in one's path, but potential sceptics may on this occasion have been denied access by the care with which the project was kept secret over five years. It is less easy to accept that one may afterwards be required to accept irksome conditions on how one practises research by an over-confident university; from about 24 February, Pons and Fleischmann might well have decided that they should put their responsibility to the scientific community before that to the organizer of their press conference. Even now, it would be interesting if they made their data generally available, whatever its correct interpretation.

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submitted to Fusion Technology 6-28-89

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Search for Cold Fusion in High Pressure D_2 Loaded Ti and Pd Metal and Hydride

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Revd. 07/11/89

Abstract

Various Ti and Pd metal and hydride preparations have been subjected to D_2 gas loading at pressures up to 2.4 kbar followed by temperature cycling. Neutron yield was monitored in an underground laboratory with a detection system with a counting efficiency of about 9.2% and a background of 10 counts/hour. No clearly identified neutron yield in excess of background has been detected.

Introduction

The reports of cold fusion based upon excess heat generation by Pons and Fleischmann [1] and neutron yield by Jones et al [2] have prompted enormous efforts to verify the effects. Several groups [3-4] have reported the observation of neutrons generated where Ti or Pd is subjected to modest D_2 pressures and thermally cycled between 300 and 77 K. The credibility of these measurements have been questioned [5] because of difficulties with the neutron background and spurious counts from gamma radiation at the very low (a few counts/hour) levels involved. In this work we describe measurements on a variety of Ti and Pd metal preparations, some of which result in full hydriding of the material, under conditions of extremely low neutron and gamma ray backgrounds with a highly sensitive neutron detection system. A particular effort was made to verify the generation of neutrons in bursts that have recently been reported [6].

10/16/89 -- Howard Menlove & Co. were unable to provide a reliable system -- Felt they were susceptible to cosmic rays.

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Experimental Details

To fully evaluate the various reported observations of neutrons in the low temperature thermal cycling of metals under moderate D_2 pressures, a large number of materials were investigated individually and in combination. Pure elements (5-9s) consisted of Pd, Ti, Zr, and V. The elements were employed in sponge form for Pd and Ti as well as bulk materials (normally prepared as filings immediately before use). The bulk materials were etched as follows: Pd (fresh aqua regia for 5-20 min.), Ti and Zr (HNO_3, HF, H_2O: 1:1:50 for 1-5 min.), and V (HNO_3, H_2O: 1:1 for 1-5 min.). Sponge materials were not etched before use. Some of the sponge was pressed into pellets at pressures from 5000 to 15000 psi using 1 cm diameter dies. Because of the porosity of these pellets, they were used as pressed. Another material investigated was TiD_2. This was synthesized from the pure metal and pressed Ti sponge pellets by heating under D_2 gas using the following schedule: The temperature was raised to 700°C over a 1 hour time period, held at 700°C for 1 hour, cooled to 500°C over 1 hour, held at 500°C for 3 hours, cooled to 300°C over 1 hour, held at 300°C for 3 hours and then cooled to room temperature over a three hour time interval. The resulting material was the stoichiometric deuteride by weight. These materials were made because of the expected inability of the experimental conditions in the bomb to effectively synthesize the deuteride.

Some of the Ti materials that had been run in electrochemical experiments [7] replicating those of Jones et al, [2] in electrolytes consisting either of his original published mixture of salts or in a simpler electrolyte of 0.1M $LiNO_3$ and 0.05M $Pd(NO_3)_2$ with the pH adjusted to 3 with HNO_3, were also used in the pressure experiment. In addition, some of the pure metals and sponge were soaked in the Jones electrolytes and then dried at 100C for 1 hour. During the soaking process some electroless deposition of Pd was observed. The electrodes were cut into pieces and placed in the pressure bomb. Some Ti was also oxidized by annealing at temperatures

between 700 and 900°C for 1 to 10 hours to investigate the possibilities of fracto or pyro fusion [6].

All of the above materials were run in the pressure vessel, initially in individual ~10 gm lots. Finally larger pressure vessels, consisting of converted lecture bottles were run to evaluate the effects of larger amounts of materials.

Pressures were generated in a gaseous D_2 system capable of pressures to 5 kbar. The pressure vessels were typically baked after loading in a rough vacuum at 150-200 C for ~1 hour to remove moisture from the material. The quantity of material at the various pressure values are listed in Table I. For the high pressure (1-2.4 kbar) runs a 1/8 inch 1D by 3/8 inch OD by 11 inch long BeCu vessel was used. The runs at 0.7 kbar were taken in a 1/4 inch 1D by 3/8 inch OD by 11 inch long BeCu vessel. The 50 bar runs were taken in a standard lecture bottle with a volume of about 200 cm^3.

Our procedure was to flush the loaded pressure vessel several times with D_2 and charge to the desired pressure. The pressurized vessel was immersed in liquid nitrogen and allowed to equilibrate. The liquid nitrogen was then poured out and the pressure vessel allowed to warm to room temperature. If this warming took place in the dewar, it required several hours to reach ambient temperature. The BeCu pressure vessels reached room temperature in about one hour when warmed in the experimental cavity without the dewar. The lecture bottle took somewhat longer.

The experimental geometry is shown in Figure 1. Two 11 counter arrays of He^3 neutron tubes in blocks of polyethylene which act as a moderator, shown end on in the figure, are placed about 13 cm apart. The arrays are 33x33x10 cm^3. One array of 11 tubes is read on one channel and 5 and 6 of the tubes in the second array are monitored on 2 other channels so that there are three independent n-counters at all times. The two arrays are covered with Cd foil to stop thermal neutrons and the whole assembly is surrounded with high density polyethylene (3-4") to provide some shielding from ambient

fast neutrons. Most of our experiments were conducted in a tunnel where the earthen overburden is at least one hundred feet.

The neutron counting system and its calibration are discussed in detail in reference [8] but briefly are as follows. The calibration was performed with a Cf^{252} (E_n = 2.1 MeV) source (traceable to NIST) placed in the location of the pressure bomb. Peak sensitivity is to neutrons in the energy range $0.1 < E_n$ (MeV) < 4. The total measured efficiency, summing the three counter channels was 0.092 counts/source neutron. The measured background was 10 ± 1 counts/hour. Therefore, the background equivalent source emission is ~ 110 n/hour. We emphasize that our configuration involves three independent channels because at this level of detection occasional noise can yield provocative signals which only such redundancy can screen.

Using this experimental configuration, the various materials were cycled up to 10 times in temperature from 300 to 77 to 300 K at the charge pressure. Many of the runs were also monitored upon release of the pressure at 77 K and subsequent warming to 300 K. A typical trace of a slow warming is shown in Figure 2 where the temperature and total number of a counts per 10 minute time interval from all three channels is shown as a function of time. Note that each of the 3 neutron channels was also monitored and recorded independently. No counts significantly above background were seen in any of our runs.

Based upon reports [6] in similar experiments of bursts of neutrons in < 128 μsec we looked for coincidences in the 3 parallel channels with 1 sec time resolution for all of the materials shown in Table I that were run at a pressure of 50 bar. Under these conditions our measured single count rate is 9.8±0.4 cts/hr. If we assume that all counting events are totally uncorrelated, then we can estimate the rate for accidental double counts and triple counts. These numbers are 0.030±0.002 cts/hr. for double counts in the approximately one second time interval and 9.6×10^{-5} cts/hr. for triple counts in the same time interval. Over an approximately 300 hr. measurement time the corresponding

measured double and triple count rates are 0.067±0.015 cts/hr. for double events and 0.003±0.003 cts/hr. for triple events. The triple event rate is based on only a single triple coincidence and thus the measured rate, statistically, is not significantly different than the estimated accidental rate. The measured double coincidence rate is a factor of two larger than the estimated accidental rate. We attribute this difference to correlated events caused by gamma rays passing through and triggering two counters simultaneously. Thus no neutron counter evidence exists for the detection of cold fusion.

We should point out however, that apparent neutron bursts have been observed a number of times in single counter channels (see Fig. 3). These bursts have been identified as coming from microphonics and internal discharges in the detector system. Thus redundancy in the neutron counter system is important to distinguish these noise events from real neutron bursts. Only those events which are observed simultaneously in our three counters with the correct relative intensities can be considered as arising from real bursts.

Conclusion

Based upon a large number of D_2 pressure-temperature cycles of a variety of Pd, and Ti metal and deuteride preparations in which we see no neutron events above a very low background, we are forced to conclude that no fusion has occurred. Because of the controversy as to whether the cold fusion is a bulk or a surface related phenomena it is difficult to assign a threshold value to the number of D-D events that could have been seen. We have however spanned very large ranges of volume of material (several 100 gms), surface area (with the powdered materials), and pressure which are comparable to those claimed in successful experiments. It should be stressed that without n-counter redundancy, reports of the irreproducible generation of neutrons, including bursts, should be treated with great skepticism.

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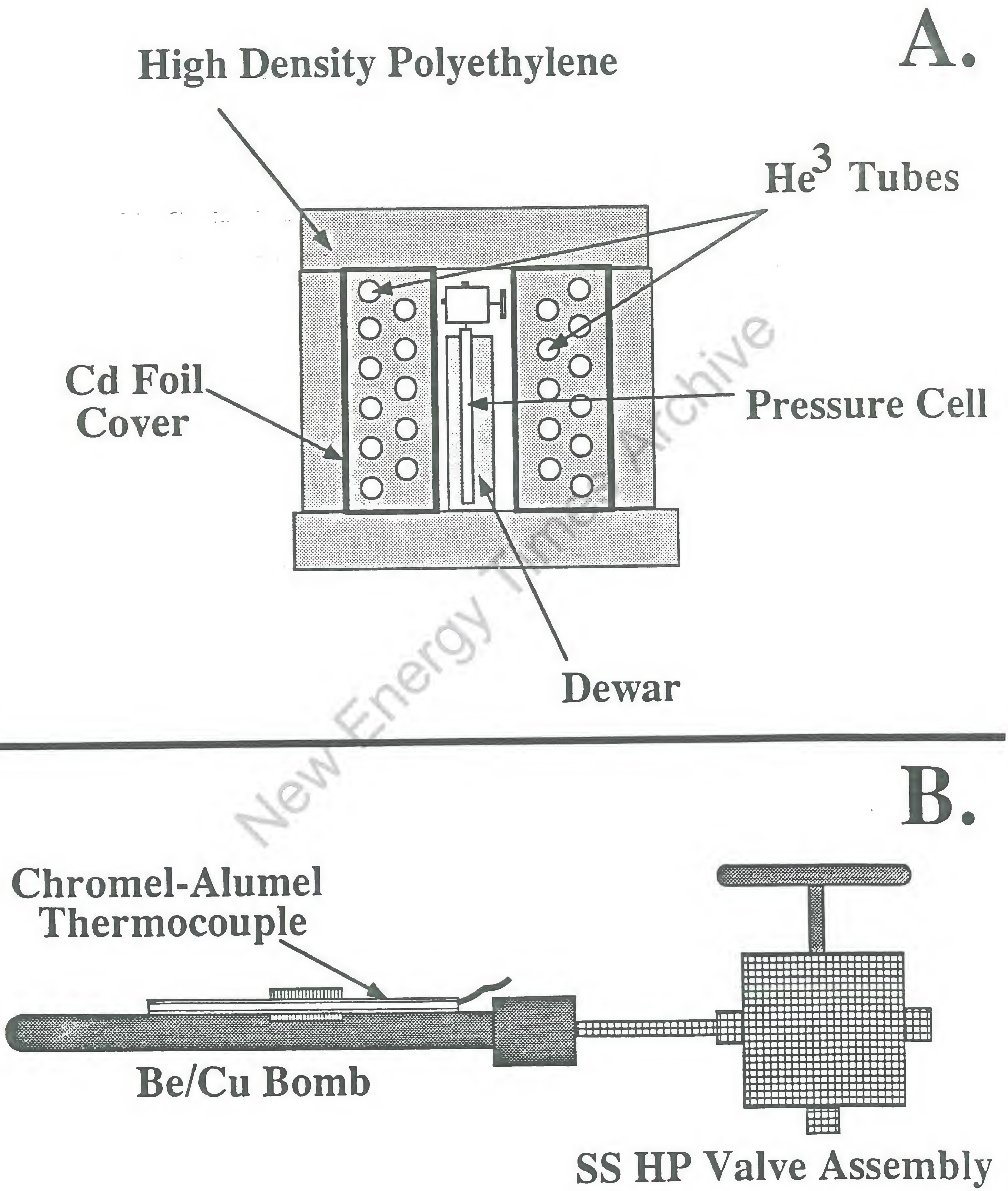
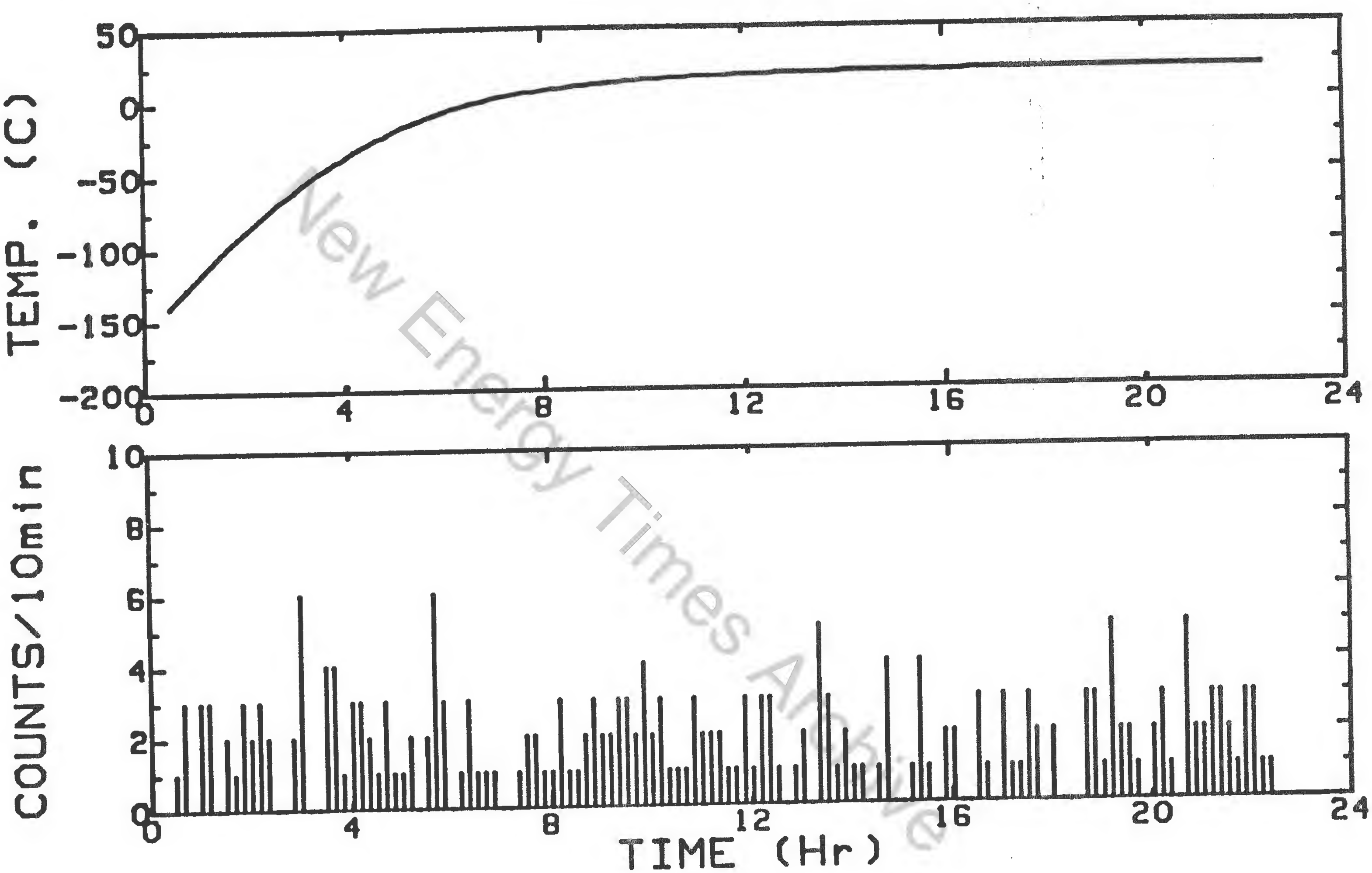
Figure Captions

1. The experimental configuration showing the position of the pressure bomb with respect to the neutron counters (A) and the details of the bomb and attached thermocouple.
2. A typical experiment when the pressure bomb is slowly warmed from liquid nitrogen to room temperature. In addition to bomb temperature, the total number of counts in all three counter channels is shown per 10 minute interval.
3. An experiment showing an anomalous burst of counts in channel 1 of the neutron counters (lower figure) when no comparable event occurs in the sum of channels 2 and 3 (upper figure). The measurement time interval is one second.

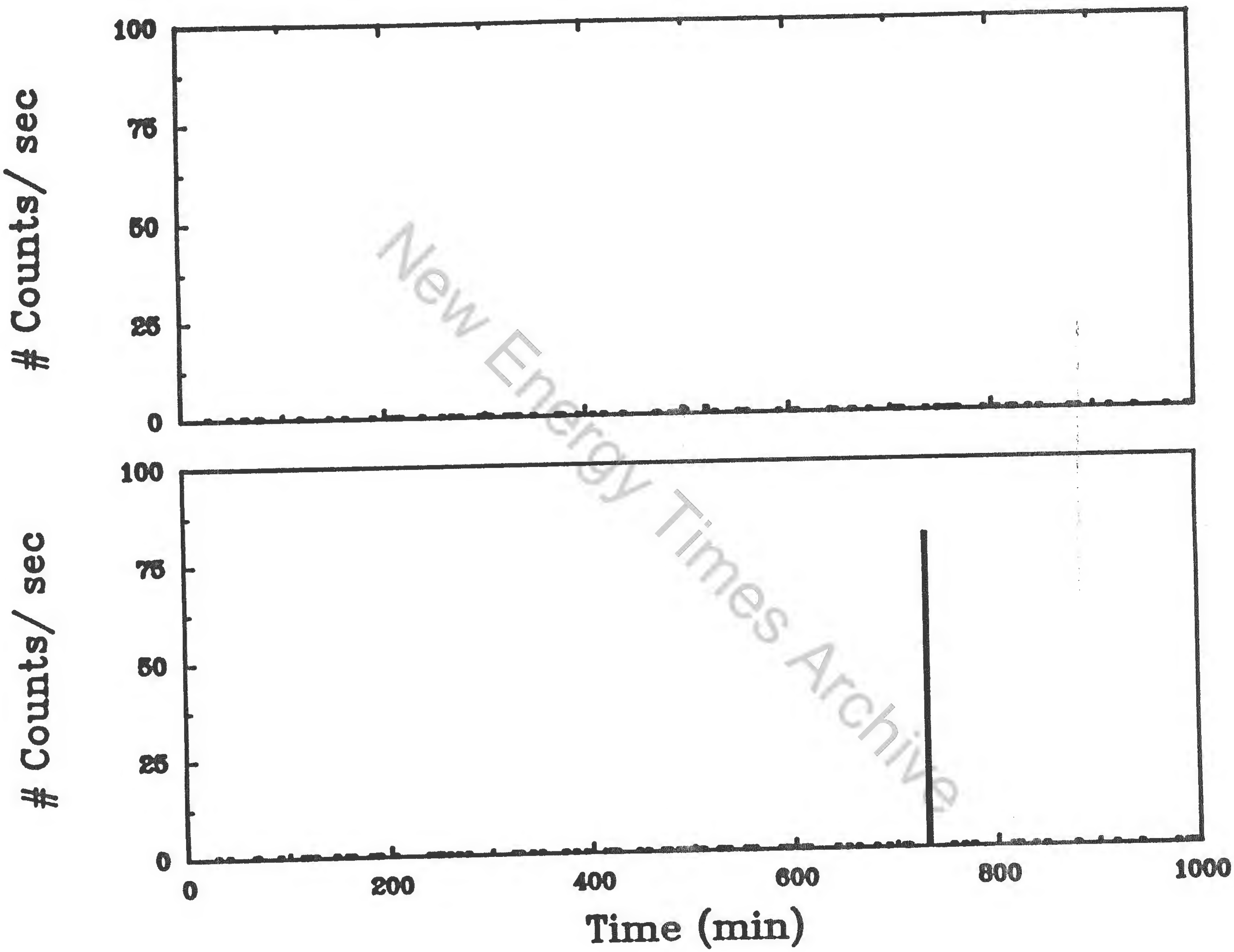
Table I. Pressure Loading and Temperature Cycling Experiments

Materials	Amount (gms.)	Pressure (bar)	Running Time (hrs.)	#Temp. Cycles
Above Ground Experiments with 14000cts/hr. Background				
Pd sponge	8.0	1700	23	1
Ti sponge	3.5	2400	7	1
Below Ground Experiments with 10cts/hr. Background				
Ti filings and sponge	10	700	3	1
Ti soaked Jones electrolyte	10 10	700 50	41 203	2 9
TiD_2	13.7 17	700 50	4 203	7 9
$Ti_{0.8}Pd_{0.2}$ air annealed 900C	48	700	6	4
Ti electrodes prev. run in Jones electrolytes	7.6	50	203	9
$Ti_{0.9}Pd_{0.1}$ Ar annealed 900C	27	50	203	9
Ti shavings air annealed 900C	23	50	203	9
Ti alloy shavings 6% Al, 4% V	200	50	203	9
Ti alloy shavings 6% Al, 6% V, 2% Sn	158	50	93	5
Zr sheet cuttings	6.5	50	203	9
V sheet cuttings	4.5	50	203	9


A.
High Density Polyethylene
He3 Tubes
Cd Foil Cover
Pressure Cell
Dewar
B.
Chromel-Alumel Thermocouple
Be/Cu Bomb
SS HP Valve Assembly



TEMP. (C)
50
0
-50
-100
-150
-200
0
4
8
12
16
20
24
COUNTS/10min
10
8
6
4
2
0
TIME (Hr)



100
75
50
25
0
Counts/ sec
100
75
50
25
0
Counts/ sec
0
200
400
600
800
1000
Time (min)

submitted to Fusion Technology.
6-28-89

HIGH SENSITIVITY SEARCH FOR NEUTRONS DURING ELECTROCHEMICAL REACTIONS

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ABSTRACT

A search for neutrons generated during cold fusion in an electrochemical cell has been conducted using a redundant detector system with three independent channels, an overall measured efficiency of 9.2%, and a background of 10 counts per hour. While spurious signals indicative of neutrons occurred one channel at a time, no real neutron emission events, where a signal is observed in all three channels simultaneously, were recorded for a wide variety of electrodes and electrolytes.

In a recent paper (1) the observation of neutron generation during the operation of an electrochemical cell was reported as evidence for the existence of cold fusion. Other investigators (2) have reported similar results in the Gran Sasso Massif where underground operation significantly reduces the neutron and gamma backgrounds. We have duplicated these experiments and attempted a number of others using a different neutron detector system with enhanced sensitivity and three independent counter channels providing a mechanism for filtering out spurious results. Although we have seen a response similar to that reported in ref. 1 in a single counter channel, the lack of such a response in all three independent counter channels simultaneously marks this response as spurious rather than real neutron emission. We conclude that at these very low counting rates a statistically significant signal must be observed in at least two independent counting channels, simultaneously, to be seriously considered as a real signature of cold fusion.

The experimental configuration is shown in figure 1. The electrochemical cell is located between two neutron detector packages each of which consist of eleven He^3 thermal neutron detection tubes embedded in a polyethylene moderator and surrounded by a Cd foil cover. The Cd foil shields the detectors from ambient thermal neutrons and shielding from ambient fast

neutrons is provided by the additional polyethylene surrounding the whole assembly. One of the detector packages is monitored as a single counter channel and the other package is split into two separate channels of 5 and 6 He^3 thermal neutron tubes each. Thus three independent counting channels are monitored simultaneously and real neutron emission events will produce a response in all three channels simultaneously with the correct relative intensities. The experiment is located in an underground laboratory on Kirtland Air Force Base in New Mexico with over 100 feet of overburden in all directions. This provides over a 200 fold reduction in the neutron background and results in an average background count rate of 10 $\pm$ 1 per hour. The detectors were calibrated with a Cf^{252} source traceable to NIST (average energy = 2.1 MeV.) located where the electrochemical cell is positioned. The total measured efficiency for all three counters together is 9.2%. This is approximately a factor of ten greater efficiency with a factor of 3 larger background than reported in ref. 1. Our system can also be operated with a time resolution as small as one second to look for coincidences that would be expected with neutron burst (3). A more detailed description of the counter system will be published elsewhere (4).

The electrochemical cell was operated under galvanostatic control using a PAR 173 potentiostat at various current densities as shown in Table I. The counter electrode was Pt mesh and no

reference electrode was employed. The voltage across the cell was monitored continuously with a data acquisition system consisting of an HP9836 computer, an HP6942A multiprogrammer and an HP3497A data acquisition unit . This system also monitored the cell temperature, ambient temperature and all three neutron counters. The various electrode materials and electrolytes used are shown in table I.

All electrode materials, which were high purity metals (>99.99%), were etched before use as follows: Pd (fresh aqua regia for 5-20 min.) and Ti (HNO_3, HF, H_2O, 1:1:50 for 1-5 min.). The Ti pressed pellets were made from sponge by pressing in a 1 cm diameter die at 5000 psi for 4 minutes. Some of the Ti pressed pellets were converted to deuteride, TiD_2, by heating in a tube furnace under D_2 gas using the following schedule: Temperature increased to 700C in 1 hour, held at 700C for 1 hour, cooled to 500C in 1 hour, held at 500C for 3 hours, cooled to 300C in 1 hour, held at 300C for 3 hours, and cooled to room temperature over 3 hours. The resulting material was stoichiometric deuteride by weight. Some of the foils were stressed by extensive hammering with a steel hammer on a stainless steel anvil. The nominal surface area of the resulting foils were increased by factors of 2-4.

Contacts were made by spot welding a Pt wire to the various electrodes and then potting the contact and the Pt lead in

non-conductive, epoxy (Hytrel clear 5 minute epoxy). If spot welding was impossible (Ti sponge and TiD_2), contact was made with silver epoxy and otherwise prepared in the same manner.

Electrolytes were prepared by simple dissolution of the various salts in high purity D_2O. For 0.1M $LiOD/D_2O$ all the reagents were handled under Ar and the solution was made by the direct reaction of high purity Li metal with the D_2O. For the other electrolytes, no stringent precautions were observed to preclude H_2O since many of the salts were hydrated and the pH was adjusted with HNO_3. All of the salts employed were high purity, analytical reagents. The electrochemical cell was fabricated from quartz and had a controlled vent for the release of the gases produced. The solutions were not stirred during the electrochemical experiments.

The experiments were run over 10-20 hour time periods based on the results reported in ref. 1. Current densities were chosen to cover as wide a range as previously reported values. No clear evidence of neutron emission was obtained in any of the experiments. It is appropriate to point out here that on at least one occasion an apparent neutron emission event was observed. Fig. 2 shows the behavior of the neutron counters for a Pd electrode in 0.1M $LiOD/D_2O$. Note that in counter 1 (bottom of figure) there is an increase in the counting rate considerably above background about the time that the electrochemical cell is

turned on and this lasts for several hours. If we only had available this one counter channel, it would be tempting to ascribe the response to cold fusion. However, as shown in the middle of the figure, the sum of counters 2 and 3 did not show a comparable effect. Therefore, this response cannot come from neutron emission but must be a spurious effect. Microphonics and internal discharges in the detector system have been identified as possible causes of such spurious effects. It is measurements under these conditions of very low counting rates where spurious effects in the counters are most likely to manifest themselves. Based on this result we believe that an increased count rate in a single fast neutron detector cannot be taken as evidence for cold fusion. The simultaneous response of at least two counting channels with the correct relative intensities is necessary for a claim of cold fusion to be taken seriously.

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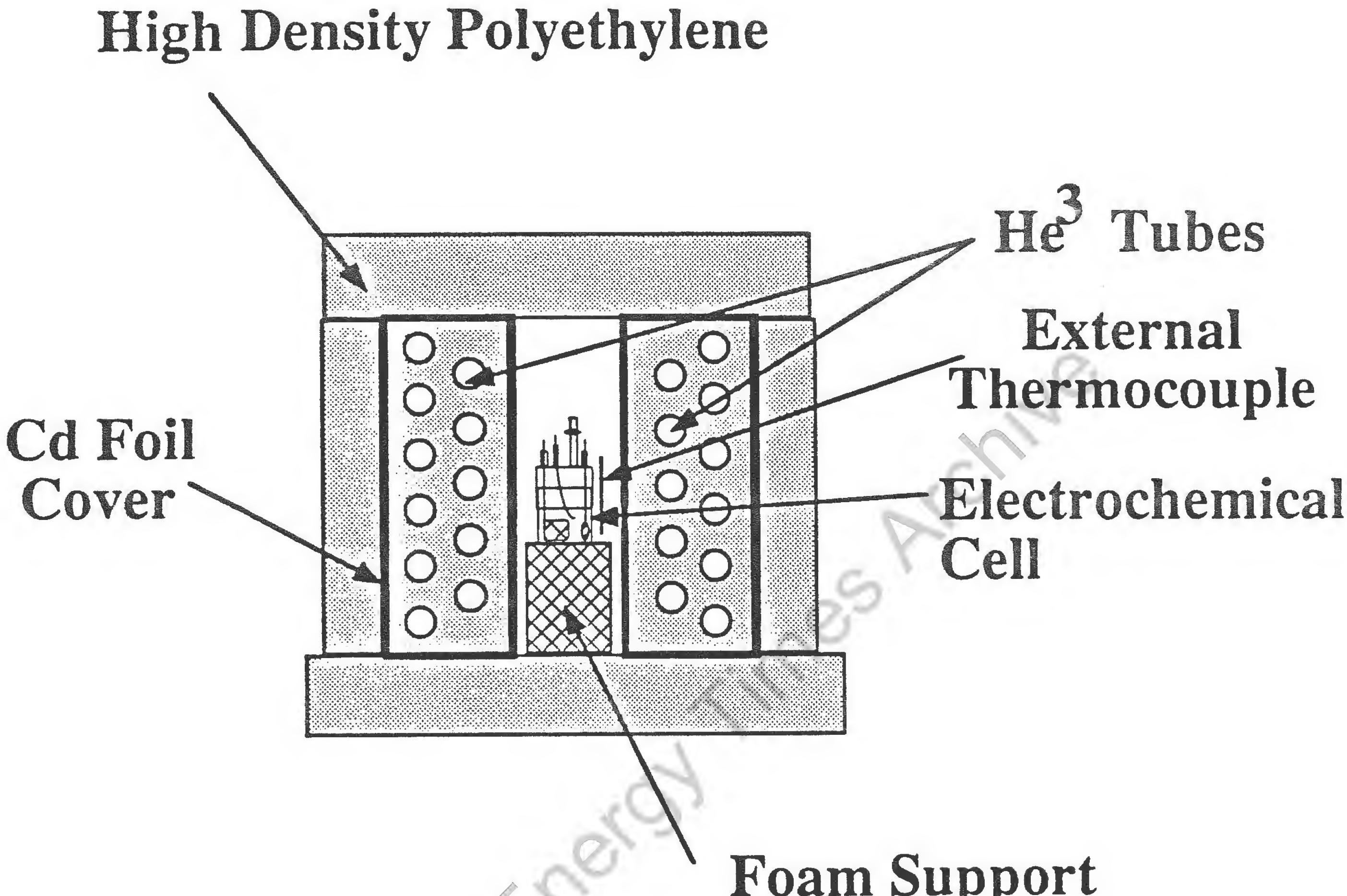
Table I Electrochemical Experiment

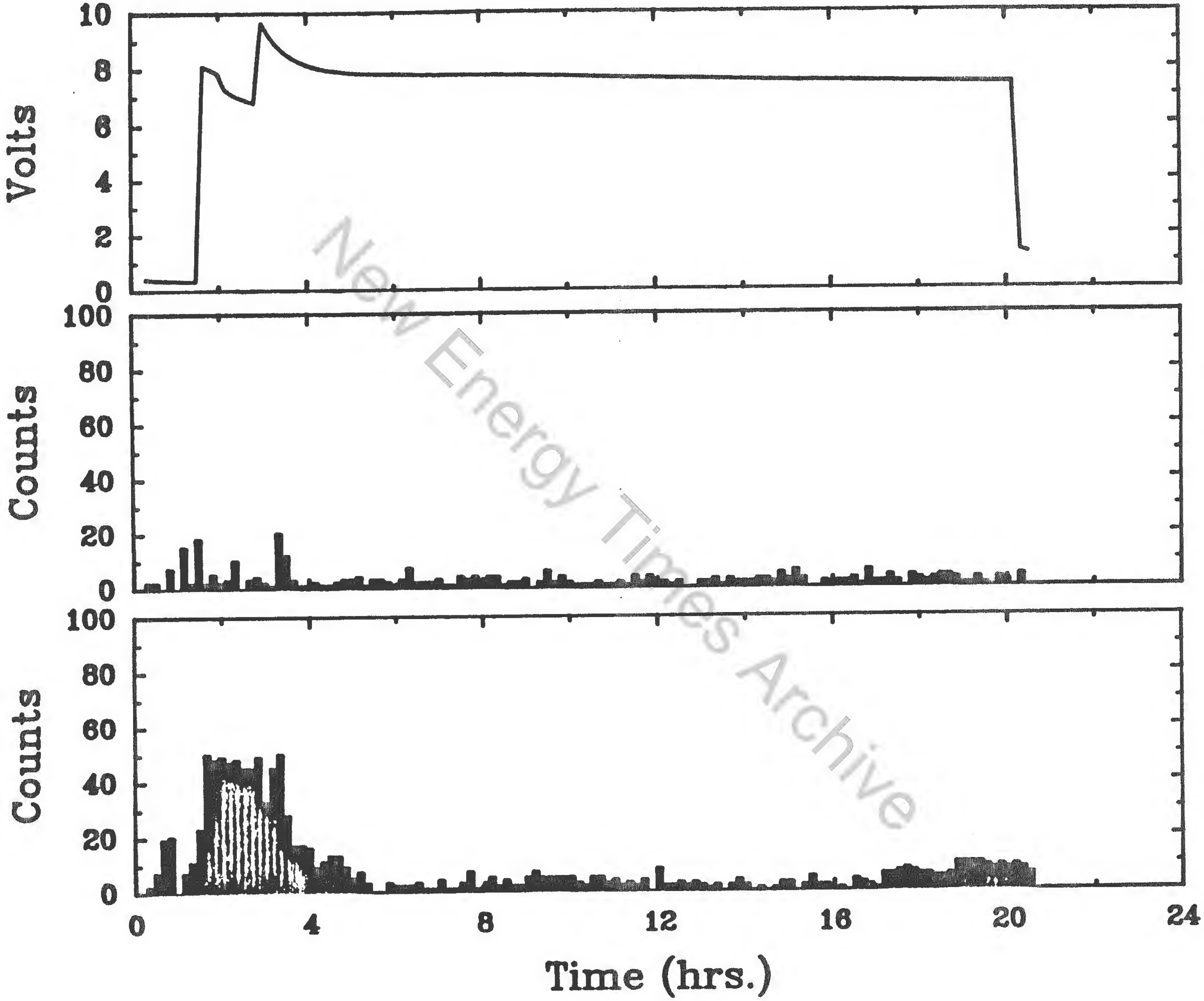
Electrode	Electrolyte	Current	Surface Area	Running Time
Cast Pd button	0.1M $LiOD/D_2O$ 0.01M $Pd(NO_3)_2$ pH adj. to 3 with HNO_3	up to 1A	3 cm^2	5 hrs.
Stressed Pd foil	0.1M $LiOD/D_2O$	up to 900mA	20 cm^2	20 hrs.
Stressed Pd foil	250 ml D_2O 0.2g $PdCl_2$ 0.11g $LiCl_2$ pH adj. to 2 with HNO_3	500 mA	20 cm^2	26 hrs.
Stressed Ti foil	0.1M $LiOD/D_2O$ 0.01M $Pd(NO_3)_2$ pH adj. to 3 with HNO_3	1A	10 cm^2	3 hrs.
Ti sponge pressed pellets	0.25g $LiNO_3$ 0.06g $Pd(NO_3)_2$ 250ml D_2O	200mA	6 cm^2	22 hrs.
TiD_2 pressed pellet	"mother earth" ref. 1	500mA	5 cm^2	20 hrs.
Pd electrodeposit on C foam	250ml D_2O 0.2g $PdCl_2$ 0.11g $LiCl_2$ pH adj. to 2 with HNO_3	500mA	6000 cm^2	19 hrs.

FIGURE CAPTIONS

1. The experimental configuration.

2. Neutron counter response and electrochemical cell voltage (top) as function of time. The lower Fig. is counter 1 while the middle Fig. is the sum of counters 2 and 3. The number of counts per ten minute interval are shown. The two steps in cell voltage correspond to applied currents of 500 mA and 900 mA. The electrode is a stressed Pd foil of approximately 20 cm^2 area. The electrolyte is described by the second entry in Table I.


High Density Polyethylene
He^3 Tubes
External Thermocouple
Cd Foil Cover
Electrochemical Cell
Foam Support



Volts
10
8
6
4
2
0
Counts
100
80
60
40
20
0
Counts
100
80
60
40
20
0
0
4
8
12
16
20
24
Time (hrs.)

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Calculated fusion rates in isotopic hydrogen molecules

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COLD fusion occurs when two nuclei with very small relative energy tunnel through their mutual Coulomb barrier to initiate a nuclear reaction. The phenomenon is well studied in muon-catalysed fusion[1–4], where a relatively massive muon replaces an electron in a diatomic molecule of hydrogen isotopes, enhancing the binding and producing cold-fusion rates of $\sim 10^{12}\,s^{-1}$. Cold fusion is also believed to occur as pycno-nuclear reactions in certain astrophysical environments[5]. Recent reports of cold fusion between hydrogen isotopes embedded in palladium[6] and titanium[7] have prompted us to reconsider previous estimates of the cold-fusion rates for free diatomic isotopic hydrogen molecules. In particular, we have calculated rates in diatomic hydrogen molecules of various isotopic composition. An accurate Born–Oppenheimer potential was used to calculate the ground-state wavefunctions. We find that the rate for d+d fusion is $3\times10^{-64}\,s^{-1}$, some 10 orders of magnitude faster than a previous estimate. We also find that the rate for p+d fusion is $10^{-55}\,s^{-1}$, which is larger than the d+d rate because of the enhanced tunnelling in the lighter system. Hypothetical enhancements of the electron mass by factors of 5–10 would be required to bring cold-fusion rates into the range of recently claimed observations.

Consider a free diatomic molecule composed of two hydrogen nuclei (which might be different isotopes). In the Born-Oppenheimer approximation the fusion rate Λ is proportional to the probability that the nuclei are very close together:

$$\Lambda = A|\Psi(\rho)|^2 \tag{1}$$

where Ψ is the normalized wavefunction describing their relative motion. The internuclear separation ρ is a typical distance at which nuclear interactions occur, approximately 10^{-14} m (10 fm).

The nuclear rate constant A for a given pair of nuclei is related to the low-energy behaviour of the corresponding fusion cross-section. If the variation of the cross-section $\sigma(E)$ with relative energy E is parameterized in terms of the usual S-factor,

$$\sigma(E) = \frac{S(E)}{E}\,e^{-2\pi\eta}, \qquad \eta = \frac{e^2}{(2E\hbar^2/\mu)^{1/2}} \tag{2}$$

where μ is the reduced mass of the two nuclei and e is the charge on the electron, then

$$A = \frac{S(E=0)}{\mu c^2}\,\frac{c}{\pi\alpha} \tag{3}$$

with $\alpha = e^2/\hbar c \approx 1/137$. Table 1 shows the nuclear rate constants for five possible interactions between two hydrogen nuclei[8].

We restrict ourselves only to s-wave nuclear motion (for which the fusion rate will be largest), so that the wavefunction can be written as

$$\Psi(r) = \frac{\psi(r)}{(4\pi)^{1/2} r} \tag{4}$$

with the normalization $\int_0^\infty \psi^2\,dr = 1$. The radial wavefunction ψ then satisfies the Schrödinger equation

$$\left[-\frac{\hbar^2}{2\mu}\frac{d^2}{dr^2} + V(r)\right]\psi(r) = \varepsilon\psi(r) \tag{5}$$

where ε is the eigenvalue for relative motion. Unless otherwise specified, we will hereafter work in atomic units ($e^2 = \hbar = m_e = 1$), so that all energies are measured in hartrees (≈ 27.2 eV) and all distances are measured in Bohr radii ($a \approx 0.53\times10^{-8}$ cm).

Simple considerations determine the general features of the potential $V(r)$. If energies are measured relative to the energy of two isolated hydrogen atoms ($= -1$ in these units), V vanishes at large r. Furthermore, it must have a minimum of the depth and separation required to support the observed molecular bound states. At small separations, the electronic structure is that of the He atom with an energy of $V_0 = -1.9037$, so that

$$V(r\to 0) \to \frac{1}{r} + V_0 \tag{6}$$

An estimate of the suppression of the fusion rate by tunnelling is given by the barrier penetration factor obtained from the Wentzel-Kramers-Brillouin (WKB) approximation to equation (5):

$$B = \exp\left[-2\int_0^{r_0} k(r)\,dr\right] \tag{7}$$

where the local wavenumber is $k(r) = [2\mu(V(r)-\varepsilon)]^{1/2}$ and the integral extends to the classical turning point, r_0.

To estimate the barrier penetration integral (7), we have taken for the diatomic molecular potential $V(r)$ the best available numerical calculation in the Born-Oppenheimer approximation, due to Kolos and Wolniewicz[9,10] (K-W). For $1.1 < r < 3$ this potential is well approximated by the Morse potential

$$V(r) = 0.1745[e^{-2.08(r-1.4)} - 2e^{-1.04(r-1.4)}] \tag{8}$$

For smaller values of r we fitted the calculated values of $V - 1/r$ to a seven-term Lagrange interpolation formula. Numerically evaluating the integral using the exact d+d eigenvalue and turning point, we find

$$B = e^{-4.13\sqrt{\mu}} \tag{9}$$

The numerical coefficient of 4.13 is to be compared with the estimate of 3.0-3.3 made by Zeldovich and Gershtein[2]; the difference leads to a penetration factor that is 15-21 orders of magnitude smaller.

An accurate evaluation of the cold-fusion rates can be obtained by a direct numerical integration of the Schrödinger equation (5) with the K-W potential, as shown in the second column of Table 2. The nuclear radius was taken to be $\rho = 2\times10^{-4}$ (≈ 10 fm). The numerical methods of ref. 11 were used, with the wavefunction being treated explicitly only for $r > 0.005$; the regular s-wave Coulomb function was used to extrapolate this solution to $r = \rho$.

The exact dependence of the barrier factor on reduced mass that we extract from these results in in good agreement with the WKB approximation (7), but is not consistent with refs 2 and 12. The estimate of the d+d fusion rate made in ref. 12 is too low by about 10 orders of magnitude, because in the calculation of the WKB penetration integrals an unshifted Coulomb potential was used at small separations (equivalent to our equation (6) with $V_0 = 0$). The effective energy with which the nuclei strike the Coulomb barrier is therefore lower than it should be, and hence the calculated fusion rate is smaller. We note that the p+d fusion rate is 8.5 orders of magnitude larger than the d+d

TABLE 1 Rate constants for fusion of hydrogen isotopes

Reaction	$S(E=0)$ (MeV barn)	A ($cm^3\,s^{-1}$)
$p+p \to {}^2H + e^+ + \nu_e$	3×10^{-25}	8×10^{-40}
$p+d \to {}^3He + \gamma$	2.5×10^{-7}	5.2×10^{-22}
$p+t \to {}^4He + \gamma$	2.6×10^{-6}	4.8×10^{-21}
$d+d \to {}^3He + n \oplus {}^3H + p$	1.1×10^{-1}	1.5×10^{-16}
$d+t \to {}^4He + n$	1.1×10^{1}	1.3×10^{-14}

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TABLE 2 Cold-fusion rates in isotopic hydrogen molecules

	$m^*/m_e=1$	2	5	10
p+p	−64.4	−48.0	−33.2	−25.6
p+d	−55.0	−36.0	−19.0	−10.4
p+t	−57.8	−37.7	−19.7	−10.5
d+d	−63.5	−40.4	−19.8	−9.1
d+t	−68.9	−43.5	−20.9	−9.4

Cold fusion rates are expressed as $\log_{10}$ of the rate in s^{-1}.

rate. Although the nuclear rate constant for p+d is 5.5 orders of magnitude smaller than for d+d, the smaller reduced mass in the former case enhances the tunnelling probability more than enough to compensate for this. This same effect allows the weak p+p fusion to compete favourably with the strong d+t reaction.

We have also calculated fusion rates in other states of the hydrogen molecule. For example, the p+d and d+d fusion rates in the second vibrationally excited state are enhanced by 2.5 orders of magnitude relative to the ground-state values. Furthermore, the d+d fusion rate in the ground state of the molecular ion was calculated using the exact Born–Oppenheimer potential tabulated in ref. 13 to be smaller than that of the molecular ground state by 6.5 orders of magnitude. For the general molecular-ion ground state, the coefficient 4.13 in equation (9) changes to 4.80.

It is interesting to ask by how much the internuclear separation must be decreased to reach the fusion rates claimed in refs 6 and 7. Although the precise answer depends on the details of the internuclear potential, a simple way of quantifying the problem is simply to change the radial scale. This is equivalent to endowing the electron with a larger mass m^* than it actually has, in which case the separation scales as m_e/m^*. (This enhanced mass should not be associated with any physical excitation in a solid material, as only the bare electron is relevant at the short length scales that are important here.) Accurate estimates can be made by numerical integration of the Schrödinger equation (5) with the K–W potential[9,10] as shown in Table 2. For other values of m^* we have found that a very good fit is given by

$$\log_{10}\Lambda = 6.5+\log_{10}(A/a^3)+3\log_{10}(\mu/M_n) - 79(\mu/M_n)^{1/2}(m_e/m^*)^{1/2} \qquad (10)$$

where M_n is the nucleon mass and the variation with m^* is that given by equation (9). A mass enhancement of $m^*\approx 5m_e$ would be required to bring the cold-fusion rates into the range claimed in ref. 7. This is 2.5 times larger than the value calculated previously[2,12]. An enhancement of $m^*\approx 10\,m_e$ is required by the results of ref. 6.

It is worth remarking on the validity of the Born–Oppenheimer approximation as used in this context. As there is a large difference between the potential and total energies in the classically forbidden region, one might naively expect a failure of the adiabatic assumption. However, more careful reflection suggests otherwise. Systematic corrections to the adiabatic approximation are possible by considering the full coupled-channels generalization of equation (5)[14]. The largest coupling terms are of order

$$\frac{1}{\mu}\left\langle n\left|\frac{\partial}{\partial r}\right|m\right\rangle\frac{d\Psi_m(r)}{dr}\approx\frac{k(r)}{\mu}\Psi_m(r) \qquad (11)$$

where n, m label the electronic states, which we assume to vary on the scale of the Bohr radius. The local wavenumber at small distances is $k(r)\approx(2\mu/r)^{1/2}$. This term is to be compared with the effect of the diagonal potential at short distances, $\sim\Psi_m/r$, giving a correction to adiabaticity of the order of $(k/\mu)/(1/r)\approx(r/\mu)^{1/2}\ll 1$.

These calculations of the fusion of hydrogen nuclei embedded in a metal are only approximate. Screening by the electrons modifies the Born–Oppenheimer potential[15]. Moreover, fluctuations present in many-body situations might significantly enhance fusion rates[16], although there are limits to the efficacy of this mechanism in equilibrium conditions[17].

In summary, we find that the rate for d+d fusion in the free hydrogen molecule ($\sim 3\times10^{-64}\,s^{-1}$) is 10 orders of magnitude larger than the most recent previous estimate[12], and that the rate for p+d is faster still by 8 orders of magnitude. The latter remains true for rates slower than $\sim 3\times10^{-17}\,s^{-1}$ ($m^*\approx 6m_e$). Hence, if in the experiments of ref. 7 any nuclear process is occurring at all, the neutron-free p+d reaction could be playing a role. We also find that hypothetical enhancements of the electron mass by factors of 5–10 are required to bring cold-fusion rates into the range of values claimed experimentally. However, we know of no plausible mechanism for achieving such enhancements.

After submission of this manuscript, we became aware of similar calculations by H. Picker[18] that agree in part with our results. □

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ts on DOE Laboratory Results

Steve May

Attached are highlights from DOE laboratories reporting on cold fusion research for the week ending June 23. Copies of the full reports are on file in my office.

There continues to be no evidence in DOE laboratory experiments of cold fusion neutrons or excess heat that would support claims of the discovery of a new energy source. LANL continues to observe low levels of neutrons, the origin of which remains unexplained. A Los Alamos scientist spoke to a reporter about a finding of tritium in one of his experiments. This appeared in the Salt Lake City Deseret News on June 23, 1989 and then appeared in a New York Times article of June 27, 1989. However, the finding is not reproducible and Los Alamos officially is skeptical and is not reporting it to us yet as a LANL result.

Activities at most laboratories have slowed or stopped. Only LANL, ORNL and PNL reported this week on experiments, which are proceeding.

PNL was approached by the University of Utah to serve as a "mediator" in the analysis of Pons' electrodes for the presence of helium-4. If negotiations are successful, PNL would coordinate the analysis of 5 to 7 electrodes provided by Utah among a number of independent laboratories selected by Utah. PNL would also assist in the interpretation of the data.

The ERAB Cold Fusion panel met in Forrestal on June 22, 1989, to review the evidence presented to date. The panel's first report is scheduled for the end of July 1989.

Without objection, it is our plan to discontinue these weekly reports and to report, instead, on as warranted basis.

Robert O. Hunter, Jr.
Director
Office of Energy Research

Attachment

CONCURRENCES

RTG SYMBOL	INITIALS/SIG.	DATE
ER-1	L. Ianniello	6/27/89
D. Mayhew	ER-61	6/ /89
ER-2	J. Decker	6/27/89

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ER-1 (Robert O. Hunter, Jr., 586-5430)

Week #9 of Cold Fusion Highlights on DOE Laboratory Results

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Week #9 (June 19-23) Highlights of DOE Laboratory Research on Cold Fusion

Ames Negative report.

LANL Experiments mentioned in previous reports are continuing. A new experiment was conducted which looked for correlations between acoustic emissions and neutron bursts in a gas cylinder filled with titanium shavings and deuterium gas. A sensitive microphone was attached to the exterior of the cylinder. A rapid uptake by the Ti of the gas was evidenced by falling pressure, which was accompanied by copius acoustic signals that lasted 20 hours. Single neutron bursts were periodically observed, but not correlated with acoustic variations. A similar experiment was conducted looking for ionization events and correlations with neutron bursts, with similar results. In other experiments, D/Pd ratios of 1.28 have been achieved, as measured by weight gain. These electrolysis experiments are being monitored for heat, neutrons, and tritium, with no corroborated evidence of fusion to date.

ORNL Four groups continue experiments with no evidence of cold fusion or excess heat. Highly sensitive calorimetry experiments are proceeding with all cells in energy balance (no excess heat) to within one watt.

PNL *No change.* *Calorimetry/electolysis* experiments are being redesigned to operate within a closed system. Separate studies are investigating the effects of overvoltages, electrolytic contaminents, and other factors on the uptake of deuterium by outgassed, annealed palladium. The Laboratory is continuing to discuss its possible role in serving as a mediator for the Univerisity of Utah in the analysis of several of Pons' electrodes.

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To: RLG2@YKTVMV
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Dear E632 and WA84 Collaborators, 17 June 1989.

COLD FUSION NEWS No. 15

1. INTRODUCTION

While Nathan Lewis gave possible explanations of the excess heat claimed by Fleischmann and Pons, there were always some worries if this were the full explanation. Now several other pieces of information have appeared which may help to explain the various reports of excess heat by several groups - it feels like some missing pieces of the jigsaw have turned up.

Two new important sets of results have been reported, both by friends of Martin Fleischmann. Kreysa et al. reproduced as closely as possible, the cell of F&P and do extensive calculations to show that the excess heat observed by them could be explained as coming from the catalytic recombination of the hydrogen and oxygen by palladium above the electrolyte. The recombined D2O would escape as vapour but the heat produced would be in the cell. The group at Harwell led by David Williams, has performed what is probably the world's most extensive series of experiments which tried to cover all possiblities with

good technology and good statistics. After spending 320 000 pounds sterling, they have now decided to end research into cold fusion.

Null results are reported from Ames Lab. Positive results, with varying amount of details, are reported from several labs - the Rector of a Madrid University invited me to visit them at four hours notice to discuss their results. This plus having a test run of our scintillating fibres for WA84, have slightly delayed this note - sorry.

2. RESULTS OF KREYSA et al.

Prof. G. Kreysa of the Dechema-Institute, Frankfort, and Profs. G. Marz and W. Plief of the Free University of Berlin have a paper accepted for publication by the J. of Electroanalytic Chemistry. After recalling Fleischmann and Pons's 5 reported results, they write "We have to report here that we tried to confirm these results and that in principle we could observe all the phenomena reported by Fleischmann and Pons[1]. However, by several additional check experiments, we could explain all our results without the assumption of any nuclear fusion. Furthermore we found some errors in the paper [1] and some possible errors in the experiments."

They describe their apparatus in great detail, give temperatures, times and detailed numerical calculations of the heat flow, finishing by writing "with these results the total heat production related to Joule heat is 125.7% and related to total electrical energy supplied 104.4%. However these results are erroneous. Due to the temperature difference between the ambiant and the bath, the bath takes up also heat from the ambiant." After more calculations given fully, they conclude that the above percentages should be 98% and 81.4% resp. "The small error of 2% is obviously due to heat losses via the lid of the cell." Such heat losses are not in [1]. "We have repeated such heat balances more than 10 times and never found a significant heat excess" Here the palladium cathode was completely immersed in the electrolyte.

"We have to report here that as we removed the deuterium loaded palladium sheet from the cell and laid it on the table it did burn a scald into the table plate. One still can argue that this was due to deuterium fusion. Therefore we loaded the palladium sheet cathodically with hydrogen using an electrolyte containing only normal water (no enriched heavy water) and laid it onto a piece of wood where it also burned a scald (see fig. 2)." This means that oxygen from outside the cell (i.e. air) has reacted and they say it releases 147.3 kJ/mole D. "The principle of flameless catalytic combustion of hydrogen is used in catalytic hydrogen burners (D. Berhens (ed); Wasserstofftechnologie - Perspektiven fur Forschung und Entwicklung, Dechema, Frankfurt/M 1986, p 320 ff." To be more quantatative, they laid a hydrogen-loaded sheet on to glass rods and "measured, after an incubation time of 15 s a temperature rise of the palladium from 20 to 418 degrees within 74 s". They estimate a "heat flow of 35.9 W and a heat flow density of 179.6 W cm-3."

Now there have been many reports that the observation of excess heat was difficult to reproduce, so it is interesting that Kreysa et al. said that to observe this catalysis effect, they had to first catalytically activate the surface. This they did by alternating polarization with -3 and +3 Amps in 1 mol/l sulphuric acid for several times. When there was no activation, there was no effect.

Kreysa et al. assume that for some or all the time, there is palladium above the surface of the electrolyte. They note that "the water formed by catalytic recombination at the palladium surface will leave the cell as vapour since the palladium surface is heated up due to the recombination." They then compare their hypothesis that the "excess heat" observed is due to catalytic recombination of hydrogen with the oxygen in the gas phase. They recalculate the data in F&P's table and find it possible to explain the "excess heat" claimed. The paper also does calculations which indicate that the case where the cube of palladium melted, can also be explained by catalytic recombination.

They also measured neutrons, gammas and tritium and finish by writing "In summary, the results of Fleischmann and Pons cannot be considered as a doubtless proof for the occurrence of cold deuterium fusion."

Prof Marz told me that they wished to present these results at the

Electrochemical Meeting at Los Angeles on May 8th. When they were told they could not as only positive confirming experiments could present results, he decided this was unacceptable and he would not go. However Prof. Kreysa went and at first was refused, but he told me that Martin Fleischmann said this was wrong and tried very hard to get him permission to talk. Finally he was allowed to present his results but this was at the time of the Press Conference and most of the delegates and almost all of the ones interested in cold fusion, went to it. [It should not be forgotten that at the Santa Fe Workshop the desire to see positive results caused a probably unintentional censureship - it seemed that all the positive results, no matter how feeble the experiment, were presented while many null results, some of them new and technologically excellent, were exiled upstairs to the poster session].

3. HARWELL RESULTS

On the 15th June Harwell issued a press release. Dr. Ron Bullough, chief scientist of the AEA said "The potential benefit and scientific interest in cold fusion, together with the Government's need for information and advice, meant that the subject had to be investigated. However results to-date have been disappointing and we can no longer justify devoting further resources in this area." Later it is written; "The programme started on 13 March 1989." [note this was 10 days before the Fleischmann and Pons press conference - Martin Fleischmann is a consultant at Harwell]. "The research programme used 4 million pounds worth of equipment and cost 320 000 pounds. Scores of other specialists from Harwell's 4000 strong workforce contributed expertise and equipment" to Prof. David Williams and his team of ten. "as many as 30 cells were under observation at a time and well over 100 different experiments were performed."

3.1 Measurements of Fusion Products

Firstly a neutron detector with 50% efficiency was used and a limit of one n per sec was obtained. Then a system was used where the active cell and an unpowered one were exchanged automatically every 5 min. and this gave an upper limit of about 0.05 neutrons per sec. A Pd foil was run in 4 ways, a Ti rod in 2 ways and Ti granules in 3 ways (e.g "Jones Brew"). Als
o
TiFe (a known hydrogen storage material), UPt3 and CeAl2 ("heavy electron metals") were also used as cathodes. Thirdly an assembly of 8 cells with 3He neutron detectors and counters of gammas with energy > 1.75 MeV, was used with 8 types of cathode some of which had the current cycled up and down looking for dynamic effects. Fourthly a double chamber neutron counter with good controls giving limits of a few neutrons per sec. Fifthly a large Ge detector which is a sensitive gamma detector, was used to look for 5 MeV gammas from the p-d reaction which should have a higher cross section than the d-d reaction - here Pd and Ti cathodes were used in a mixed D2O/H2O electrolyte

3.2 Calorimetry

Firstly 16 "Pons-Fleischmann" type calorimeters were constructed and 1, 2, 4 and 6 mm diameter rods from Johnson Matthey used with 4 electrolytes (Li and Na hydroxides with D and with H as control). Neutron counters were also present.

Secondly larger calorimeters with an aluminium case whose temperature was measured. This "eliminates the sloping background and temperature gradients which bedevil measurements with the Pons-Fleischmann design" - i.e. the problems with having to use the rate of cooling, are less. Four types of Pd rod were used.

Thirdly an "Isothermal" calorimeter designed for nuclear safeguards work was employed. It is large and a cell of 2 litres capacity was inserted. "Anode and cathode gases are kept separate and are lead out of the calorimeter separately" [so there was no chance of catalytic recombination as Kreysa et al. have suggested]. Any power emission from the cell is measured by the variation of the power required to maintain the chamber at a constant temperature (42 degrees C). This eliminates the sloping baseline. The resolution is +/- 8mW in 20 Watts. Five cathodes were used at various current densities.

Many conditions were tried, e.g. Pd was used with 8 different metallurgical histories.

3.3 Importance of Use of Multiple Detectors

An important point is that multiple banks of gamma and neutron detectors were used and this was found to be able to eliminate spurious events due to electronic mal-function in a single detector [many of the positive claims turn out to have had only one detector so that it was not possible to look at the recordings from other detectors at the same moment of time].

3.4 Additional Studies

Measurements were made of the penetration times of D into Pd and Ti, of the amount of loading of D as a function of the composition of the solution and of poisons of the surface hydrogen recombinatio n

The amount of deuterium in the palladium was measured by hot extraction mass spectrometry (the cathode was quickly frozen in liquid nitrogen to retain D).

4He was measured by mass spectroscopy - to calibrate, 4He ions were implanted .

In the electrolytes, tritium was measured as well as Li and other elements.

Plus other checks....

3.5 Conclusions.

David told me that the full paper should be ready in about four weeks. He said the two main results were;

1) No excess heat has been found - the power in and out balanced to about 1/2 %.

2) No evidence was found for fusion products. For neutrons the limit is at least two orders of magnitude less than Jones et al. claimed.

[Overall the Harwell claim that this is the most extensive series of experiments in the world, seems justified. And they are all null results. As The Times points out, the results coming from this lab have a special significance. "After claiming last March to have discovered cold fusion, Prof. Fleischmann returned to Harwell to advise on the experiments whose results were announced yesterday. He said that clear corroboration of his results was needed 'before the end of three months' and he was confident such backing would come from Harwell."]

4. OTHER EXPERIMENTS

4.1 Ames Lab

John Hills et al. of the Ames lab at Iowa State Univ., looked for neutrons with a BF3 detector and gammas and X-rays with a Ge detector, coming from Palladium cathodes in a D2O cell - none were seen. They tried to reproduce the "dynamic" effects seen by Scaramuzzi et al. by using powder and pieces of Titanium. The material was cycled five times between 1100 and 77 degrees K. No neutron emission above a backgrounnd of 0.01 n per sec, was seen. To look for the p-d reaction giving 3He and a gamma of 5.5 MeV, the Ge detector was used but no peak was seen. The barrier penetration factors for H-like atoms needed to account for the claimed fusion rates, were calculated but were found to be far from values typical for known metals. [Both experimentally and theoretically they find no evidence for fusion].

4.2 Institute of Physics, Beijing

Xu Yuan-ze et al. report that experiments on cold fusion have been carried out with calorimetry, neutron and gamma ray detection and with analytical chemistry methods at the institutes of the Chinese Academy of Sciences. "Strong nuclear charging emmision signals mostly with the pulsed type were recorded after some pre-charging time in some cells. Excess temperature rising was observed in some cells under large current while no abnormal heat energy was found yet in a well-designed calorimetric experiment".

4.3 Madrid Autonoma University

From the electronic grapevine, I had heard of some incidents at Madrid. At lunch-time Wednesday, was asked on behalf of th e

Rector, Prof. Lopez, to come that day to Madrid to discuss, so left at the end of the afternoon. What had happened was that Dr Carlos Sanchez and his group were using a BF3 neutron detector with a Pd/D20 cell when they started to get signals. Then the signals became very large so that being in a University, they naturally had to worry about the health safety aspect. They were advised to evacuate the region. With a large number of students being displaced, someone informed the media and they visited the university. Next day this was a front page news story.

Carlos showed me records of all his neutron, gamma and tritium data which we studied in great detail. They have now been given a second then a third neutron counter from the government lab. We discussed a variety of possible arrangement
s
and experiments. It was very enjoyable discussing with them. (Carlos had to be absent for a while on Friday as he was giving a lecture on a programme that particularly interested me, which he has developed over the last four years to teach physics to blind students - he showed me some very good practical experiments that have been developed. Does anyone know of any other university that teaches physics to blind students?). Prof. Lopez is unusually young and dynamic for a Rector - perhaps because he is elected by staff and students. Later found out he is Vice-President of the CERN Council and yet still teaches a physics class regularly.

4.4 Information from Prof. Bockris.

Prof. Bockris of Texas A&M has written to me a few times and gives some recent results that he knows about;

4.4.1 Milton Wadsworth, the Dean of Metallurgy at the University of Utah has observed "when he uses the Johnson Matthey fusion palladium, annealed at 1000 degrees in vacuum and the vacuum reduced in Argon, bursts of heat which vary from about 9 to 17 hours in length and produce much more than the energy he puts in".

4.4.2 Since Santa Fe, the Bockris group in Texas A&M has observed heat three times - "not as much as some people, but there's no doubt about it. We also find it is proportional to the current density."

4.4.3 At the Stanford Research Institute, Dr. Mackubre et al. have now "observed five large bursts of heat with the temperature going up to 20 degrees and more. Bursts last 2-6 hours."

4.4.4 At Case Western Reserve, Usi Landau et al., "is producing heat, tritium and neutrons"

[Prof Bockris has given me an example of Pathological Science working in the other direction, that is of someone finding a positive result but feeling that the local climate is not in favour of such results. So I feel that I cannot omit to mention the above four positive results even though one must be aware that no reasonably full description of how the results were obtained, nor of the numbers, nor of the statistical significance are available].

4.5 Information from the Fusion Research Lab., Universiity of Missouri

Dr. M. Prelas et al. have used the Modified Missouri Magnetic Mirror eXperimment (M4X) to search for cold fusion. They put a palladium target inside their plasma device and then introduced sub-atmospheric deuterium gas. Neutrons were counted with a BF3 detector and possibly some gammas, but write "The results are interesting but not sufficient for any conclusions at this point."

5. DISCUSSION OF UNDERSTANDING OF CLAIMS OF EXCESS HEAT.

Firstly it is not really excess heat that is being claimed but rather is an observation of excess power over a short period of time (up to 120 hours, bu
t
generally much less in an experiment that may have lasted many days or even months).

The reports of excess effects seem to be of two types;

A) sudden increases of heat flow or power for a short period of time. Fleischmann and Pons have not correlated the occurrence with any other happening. Appleby et al. of Texas A&M say the effects are correlated with changes
in particular to variations of the current

B) Prof. Huggins of Stanford appeared to be observing a steady effect in that his measurements gave temperature increases of about 10 degrees for both the cathodes of his cell with D2O and of his cell with H2O but the first was about one to one and a half degrees hotter and this was the basis of his claim to a heat excess.

Considering first B); The Tata group in Bombay similarly noted their D2O cathode was about 2 degrees hotter. Now Vincent Cate has commented that the average value for absorption of deuterium is 8379 cal/mole compared with 9605 cal/mole for hydrogen (from p125 of the "Palladium Hydrogen System" by Lewis). The consequence is that if equal energy is put in, then the cell with D2O will run hotter. He wished to use this to explain the Fleischmann and Pons results, but it may be the explanation of the results of Stanford and the Tata Institute.

Considering now A); The original experiments of Fleischmann and Pons seem to have been rather undermanned and no automatic data recording was used and the electrolyte was only sampled every two days. The experiment has been under-described in paper and talks. Nathem Lewis did a major attempt to reconstruct the cell, but it is possible that the cell he copied was not the same as was actually used. He gave a brilliant talk at the APS meeting where he tried to reconstruct the F&P calculations and was able to explain that the F&P cell was really a refrigerator that did not cool down quite as much as calculated. However this was not quite enough, and he also invoked the hypothesis that as the cell was not stirred, temperature gradients would exist so that the temperature read on a single instrument would not lead to an adequate description of energy in the cell (Prof. Meyerhof did similar calculations for Prof. Huggins' cell). However a week later F&P showed that stirring was not necessary due to the violent boiling off of the D2 and O2 gases, thus at Santa Fe little was heard of stirring except for the comment tha
t
when Prof. Huggins was doing his calibration, there were no gases and hence the temperature measurements had some uncertainty. Now the missing piece of the jigsaw may have been provided by Kreysa et al. who pointed out that catalytic recombination by palladium of the deuterium and oxygen would give a deposit of heat in the cell while the D2O vapour would leave.

This could well be the explanation as far as Fleischmann and Pons were concerned, but it does make the assumption that some palladium was above the level of the electrolyte, either the cathode or some other palladium. It is probably hard to check back on the original experiment as there is no evidence that it was tightly monitored. But for other experiments by them or other group
s
it would be good to have clear statements whether there was any palladium at any time exposed.

A possible indication has recently occurred to me, in favour of the Kreysa hypothesis that palladium was responsible for catalytic recombination. It is that, as far as I can check, all the claims of excess heat have involved palladium and none have involved titanium, though there have many claims of fission products from both titanium and palladium. Now palladium is a catalyst while titanium is not a catalyst. Is this evidence or coincidence?

6 OTHER COMMENTS

6.1 Visit of Some Members of DOE Panel to Utah.

After Prof. Pons had said the DOE Panel to study Cold Fusion contained people that might be biassed, the DOE at first postponed their visit. However it was arranged that 5 members of the panel, Messers Bard, Faulkner, Happer, Miller and Ramsey could visit U. of U. on Friday 2nd of June. They were able to have some technical discussions of calorimetry.

6.2 Los Alamos Drops Plan to Collaborate with Fleischmann and Pons

On April 27 Prof. Pons announced to the House Science, Space and Technology Committee that there would be a collaboration with Los Alamos to verify the cold fusion claims of F&P. However the International Herald Tribune of 16 June announced that Los Alamos has decided that months of foot-dragging at the University of Utah have resulted in the collapse of plans for a collaboration. The Los Alamos Director, Dr. Sig Hecker, said that in the future active negotiations could resume if the University of Utah

expressed a sincere interest and took prompt action.

Dr. Brophy of the U. of U. agreed that the university's "foot-dragging" had been a problem. "We'd still like to work with Los Alamos" he said. "I can see how they would be a little peeved. The scientists want to tell everything, but the patent attorneys tell us to say absolutely nothing".

6.3 Comments on Neutrons - M. Gai

Moshe Gai has sent me a copy of a letter submitted to Nature on "Comments on Measurements of Radiations from 'cold fusion'". Firstly he discusses the Jones et al. experiment and points out that the efficiency of their neutron detector was calculated. As he gives reasons to doubt the result, he urges that the efficiency be measured. He also asks that the statistical significance be recalculated [on 3 May, Steve Jones agreed with me that the error in estimating the background should be included in the calculation (this I estimated makes his result less than 3 standard deviations) but this was not done by his talk at Santa Fe on 24 May]. Moshe then discusses the dangers of using Pulse Height Discrimination - he says that at the start of their measurements they thought they had observed fusion, but it turned out to be 1.46 MeV gammas from 40K scattered into a second counter. He also explains why the background counting rate of 100 to 200 counts per hour observed by the Bologna-BYU collaboration in the Gran Sasso tunnel was much higher than that of about 0.1 cph found by a French group - essentially due to gammas being misidentified.

6.4 History

Mohan Kalelkar of Rutgers tells me that Maria Goppert Meyer and K.F. Herzfeld published a paper "Behaviour of Hydrogen dissolved in palladium" in 1934 (Z. fur Phys. Chemie B26 (1934) 203). Later they published a paper "On the theory of fusion", Phys. Rev. 46 (1934) 995.

6.5 Telephone Conversations with Martin Fleischmann

We spoke at some length last week and while he had read some of my earlier notes he had not had time to read the later ones but said he would do so last week-end. He had not seen most of the papers presented at Santa Fe, so I have sent him what I have of papers (with positive and null results). He said he had checked his calculations of the excess heat many times and could not find any error. When I told him of someone who was going to sell five million dollars of oil shares to invest the money in cold fusion, Martin cried out "The man must be mad". I told him I would not quote that, but Martin said I should.

6.6 Lecture on Cold Fusion at CERN

Dick Garwin gave a lecture on cold fusion at CERN where he was able to explain in more detail what he was given 5 minutes to say in Santa Fe. He also gave more detailed calculations that he had performed. He also sent me a collection of papers on Gravity Waves as claimed by Weber - this could be considered as an example of Pathological Science. Dick was largely responsible for clearing up the question and the papers he sent make fascinating (and familiar) reading. He was in Geneva for a Pugwash meeting - they were discussing defensive weapons - is there a purely defensive weapon?

Douglas R. O. Morrison.

LEP COMMITTEE MEETING

Last week there was a meeting of the LEP committee. Emilio Picasso gave a description of the progress of the LEP construction. It was a very short talk as everything was going so well there was not much to say. It is still planned that the first tests of sending electrons round the tunnel will on July 15. To allow the four experiments to be tested there will be a short test run of about three days early in August. Collider runs for physics should start in September. Dick Taylor reported on first results from SLAC. They have run at 92.2, 91.7 and 89.2 GeV where they observed 11, 31 and 3 Z0 events resp. All of these were hadronic except at 91.7 Gev

where there was one e-e, two mu-mu and one tau-tau events. [This would seem to indicate that the Z0 mass is close to 91 GeV which is slightly lower than UA1 and UA2 found but is well within their quoted errors]. The SLC machine performance is improving steadily.

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-R.L. GARWIN-
30 JUN 89 15.83

AT&T
Bell Laboratories

6/28/89

R.L. Garwin

Dick:

Here is a preprint on our electrolysis work, as reported in Santa Fe.
More to follow on gas ("burst") experiments.

regards,

Thijs Broer

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Search for Neutrons from Deuterium–Deuterium Nuclear Reactions in Electrochemically Charged Palladium

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Abstract

We report on a search for neutrons from dd fusion in Pd rods loaded electrochemically with deuterium. The rods were held in two electrolytic cells (D_2O (99.5% D)+ 0.1 M LiOD) and placed before a 12.5 dia. x 12.5 cm NaI(Tl) spectrometer system. The apparatus was housed within a polyethylene (PE) moderator, which was shielded on the outside with Pb and borax. A pair of plastic scintillator plates above and below the NaI(Tl) vetoed cosmic muons. Fusion neutrons are moderated inside the PE housing, creating a slow neutron gas that can be detected by two γ–ray producing reaction signals: (npγ) capture in the PE (2.224 MeV γ) and thermal neutron capture by ^{23}Na and ^{127}I, yielding γ–rays in the 3.5–7 MeV range. The background in the latter region is far less than the background below 3.3 MeV, which is dominated by natural ambient radioactivity. The overall neutron detection efficiency (measured with an Am–Be source) of this system is 0.015. In a four–week measurement we observe <0.007 n/sec/g Pd, compared to 2.6×10^{3} n/sec/g Pd, claimed recently by Pons, Fleischmann, and Hawkins. Our result implies $<2.2\times10^{-24}$ (ddn) fusions/dd pair/sec, as compared to $\sim 10^{-23}$ (ddn) fusions/dd pair/sec observed by Jones et al in a Ti electrode.

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Search for Neutrons from Deuterium–Deuterium Nuclear Reactions in Electrochemically Charged Palladium

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I. Introduction

The recent reports by Pons, Fleischman, and Hawkins[1] (PFH) and Jones et al.[2] of the observation of neutrons from palladium (titanium in Jones' case) electrochemically loaded with deuterium has stimulated intense interest in the verification of this process, and in the entire possibility of "cold nuclear fusion". In this paper we report our search for neutrons from the electrochemically loaded Pd–D system at room temperature.[3] We set an upper limit on the deuteron–deuteron fusion rate of 2.2×10^{-24} (ddn) fusions/dd–pair.sec, with ddn indicating the neutron producing branch of dd fusion. This is five orders of magnitude below that reported in PFH and approximately a factor of five below that first reported by Jones. One unique aspect of our experiment, is the use of the high energy portion of the gamma ray spectrum from thermal neutrons captured by Na and I in a NaI(Tl)–spectrometer system. Such a method renders the detection system relatively insensitive to ambient natural radioactivity and results in enhanced sensitivity for thermal neutrons. We also report a quantitative nuclear reaction analysis of the deuterium loaded Pd rod to estimate the amount of absorbed deuterium.

II.1 Electrolysis cell

We investigated the possible neutron emission from three Pd rods: two wires (0.125 cm diameter by 10 cm long) were mounted separately and parallel to each other in one electrolysis cell. A second cell contained a 0.4 cm diameter x 10 cm rod. The cell used with the 0.4 cm rod is shown in Fig. 1; the second cell for the two wires is similar in design. Both cells were made from borosilicate glass (Pyrex, approx. 0.2 cm wall thickness) and are sealed with Teflon or a rubber stopper to prevent D_2O exchange with atmospheric light water. The electrolytically generated O_2 and D_2 were released through a single bubbler trap, filled with silicone oil. The Pd cathode emerged from the electrolytic solution and then through the seal on the top for electrical connection. In the case of the cell with the two wires, the wires were strapped to a glass rod to prevent electrical shorting caused by shape distortion due to the deuterium loading. The submerged length was 6.9 cm for the 0.4 cm rod and 9 cm for the two smaller wires. The anode was a Pt wire (0.076 cm diameter), coiled onto a 2 cm glass tube cage. This cage was positioned coaxially with the Pd cathode and one of the glass support tubes was fed through (with the Pt wire inside) the top seal for electrical connection.

The temperature in the cells was monitored with conventional mercury thermometers. Since the purpose of this experiment was the detection of neutrons and not a quantitative measurement of any possible "excess" heat effects associated with cold fusion, these thermometers only served to indicate any gross thermal excursions. The electrolytic solution in both cells was identical to that used by PFH: 0.1 molar LiOD in D_2O (99.8 atom % D) with the LiOD obtained by adding Li_2O ($^6Li/^7Li$ ratio unknown) to the heavy water. Both cells were operated in a constant-current mode (separate

power supplies) with current densities of 63 and 70 mA/cm^2 for the 0.4 cm rod and the 0.125 cm wires, respectively. Corresponding applied cell potentials varied between 5 and 6 V. The current density values were chosen as an intermediate value of those used by PFH, at which they claimed to have observed a significant neutron flux ($4x10^4$ s^{-1} for a 0.4x10 cm rod).

II.2 Sample preparation

The Pd wires and the rod in this experiment were prepared in different ways. The two wires were cut from an available wire spool (99.95% Pd), which had been drawn to its final diameter from an ingot. One of the wires was subsequently annealed at 900 ^oC for approx. 1 hr in flowing N_2. The rod was cast from Pd powder (99.95% Pd), which was melted under Ar in a radio frequency heated BN crucible and subsequently cold rolled to its final diameter. The rod was then annealed as described for the wires. Finally, both wires and the rod were cleaned with a light mechanical polish (sandpaper). After approximately one day's exposure to room atmosphere, they were inserted in the D_2O/LiOD cell electrolyte.

II.3 Neutron detection

In this experiment neutrons are detected with a single NaI(Tl) scintillation detector (12.5 cm diameter x 12.5 cm length) which is completely surrounded with polyethylene moderator, as is shown in Fig. 2. The two cells are placed inside this moderator "house" with 5 cm polyethylene between the detector and the cells. Lead blocks were positioned outside this "poly house" to reduce undesired γ background and approximately 5 cm thick borax containing boxes were placed outside the lead shield to absorb any external neutrons. Any neutrons resulting from dd fusion reactions inside the Pd rods

are moderated to thermal energies in the polyethylene blocks. The thermal neutrons than either yield a 2.225 MeV γ–ray from the (npγ) capture reaction in the polyethylene or a virtually continuous spectrum of high energy γ's in the range 3.5–7 MeV from thermal neutron capture by ^{23}Na and ^{127}I within the detector.

The advantage of detecting in this high energy region, rather than directly at the 2.225 MeV line, is the significantly reduced background, which is mainly due to natural radioactivity. This can be seen clearly in Fig. 3, which shows a typical pulseheight spectrum from the NaI detector. Three lines, at 2.120, 2.204, and 2.447 MeV, appear in this 2.225 MeV region and are associated with the ^{238}U decay chain. In addition, background lines at 2.614 MeV (^{232}Th chain), at 1.764 and 1.847 MeV (^{238}U chain), and the ^{40}K line at 1.460 MeV were observed. The highest energy γ–ray from ambient natural radioactivity is 3.35 MeV. Thus at higher energies (>3.5 MeV) the spectrum is free from these radiation sources except for very weak high energy fission γ–rays and a very low level of α–rays from U and Th decay in the crystal itself. Thus, in contrast to the use of 2.2 MeV γ's as an indication of the (npγ) reaction, our method practically eliminates the possibility of mistaking a background γ–ray for a neutron signal.[4]

The residual background above 3.5 MeV is nearly all due to cosmic ray muons passing through the crystal. In order to reduce this background, two plastic scintillators were installed above and below the NaI detector. Events detected in the NaI crystal were gated in an anti–coincidence mode by pulses from the two plastic scintillators. The gated output for a fixed lifetime, typically 1800 sec, was analyzed and stored in a multichannel analyzer. The spectral data (0–7 MeV) was periodically transferred onto a hard disk of a personal computer. From this experimental database the

temporal behavior of the gamma ray intensities in any region can be accessed and examined; regions of particular interest are those around 2.2 MeV (region R2) and the n−signal range 3.5−7 MeV (region R1).

The neutron detection efficiency was measured with a Am−Be source with a nominal neutron flux of 6×10^4 s^{-1}. After placing this source in a identical $LiOD/D_2O$ cell in the same position as the electrolysis cells, the calibration spectrum was obtained (Fig. 3). This shows clearly the 2.224 MeV line due to the ($np\gamma$) capture, as well as the broad band signal from the reaction of thermal neutrons with ^{23}Na and ^{127}I. The line at 4.4 MeV (and part of the lower energy single escape line peak at 3.9 MeV) are due to (α,$n\gamma$) reactions in the source and from the reaction $^{12}C(nn'\gamma)$ in the polyethylene by neutrons of energy > 4.4 MeV. This feature is not expected from 2.5 MeV fusions neutrons. From this measurement the overall neutron detection efficiency from the signal in the 3.5−7 MeV region was estimated to be 0.015.

III. Results

The 0.4 cm rod was charged for 33 days during which the neutron detection system was operative. At the end of this period the current was discontinued. The sample was allowed to outgas for 24 hours in an open, silica container which was flushed with dry nitrogen gas at atmospheric pressure. After this period the 0.4 cm rod was removed and stored in liquid nitrogen. Since the diffusion coefficient of D in Pd at 77K is approx. 10^{-17} $cm^2.s^{-1}$, vs. $\sim 3 \times 10^{-7}$ $cm^2.s^{-1}$ at 300K in the β−phase of Pd−D (using the known activation energy[5−9]), storing at 77K assures retention of the remaining deuterium.

It was noticed upon removal of this rod from the electrolysis cell that the submerged section had clearly expanded and that the rod thus had a "wine bottle shape". The submerged part was approx. 0.45 cm in diameter, while the unsubmerged part retained the original 0.40 cm diameter. The rod was removed from the liquid N_2 for about two hours for dicing into discs for subsequent analysis. This process consisted of using a diamond saw to cut three, 0.15 cm discs from both ends of the rod. The different material properties of the deuterated and undeuterated portions were already noticed at this stage, as the deuterated portion cut much more slowly and unevenly. This clearly reflects the brittling effect of the deuterium on the Pd.

The deuterium loading was examined via nuclear reaction analysis using the $d(^3He,p)\alpha$ reaction, a standard ion beam analysis technique[10]. The resulting spectrum is shown in Fig. 4 and indicates a strong proton and alpha particle signal, indicative of a large deuterium concentration. Since the cross section for the analysis reaction is about 10^{-3} of the ^3He elastic (Rutherford) scattering reaction, it is immediately obvious that the deuterium concentration is comparable (within an order of magnitude) to the Pd concentration. It should be noted that the measurement was carried out at the center of the disc indicating that the deuterium had diffused through the entire cross section of the rod during the electrochemical loading time. This diffusion (charging) time to reach the rod center is $\Delta t \simeq R^2/D$, with R the radius of the rod and D the diffusion coefficient of deuterium in Pd. For R= 0.2 cm and D= 3×10^{-7} $cm^2.s^{-1}$ (at room temperature), this time is 1.5 days. Analysis of the portion of the rod which was not submerged gives no indication of a deuterium signal. The absence of deuterium 4 cm above the loaded area is consistent with a diffusion length of only 0.85 cm for the sample at room temperature for four weeks. The analysis of the nuclear

reaction data is complex since the experiment probes through the penetration depth of the ^{3}He (about 2 μm) with a reaction cross section strongly varying with ^{3}He energy. Furthermore the first few microns have lost deuterium due to out–diffusion into the atmosphere during the two hours of exposure for cutting and analysis (corresponding diffusion length ~460 μm). Nevertheless the extracted D/Pd ratio is 0.38 which is consistent with typical maximum loading values[5,9,11] and out–diffusion during the cutting period. Despite some uncertainties in this analysis, substantial deuterium loading at the center of the 0.4 cm Pd rod is clearly demonstrated.

As mentioned previously, during electrolysis the γ–ray flux both at the 2.2 MeV region (region R2) and in the region between 3.5 and 7 MeV (region R1) was measured. Fig. 5 shows the count rate with both cells inside the poly "pit" up to 14 days after the start of the experiment. At day 16 the current through the cell with the two wires (cell 2) was discontinued, the cell removed and the shielding temporarily disturbed in order to facilitate the repositioning of the cell with the 0.4 cm rod in front of the detector. As can be seen in the figure this resulted in a small but clearly observable spike in the 2.2 MeV region counting rate due to enhanced exposure to ambient room background, clearly illustrating the danger of relying on this spectral region to determine any low level, γ's from (npγ) reactions. On the other hand, no such spike is seen in the n–signal region R1.

Counting rates in region R1 between days 10 and 33 after the start of the experiment (t=0) are shown in Fig. 6. The counting rate is almost an order of magnitude lower than in Fig. 5. From day 16, only cell 1 (with the single rod) continued to be monitored up to day 30. During this period the observed signal plus background rate was 0.1472 ± 0.0004 s^{-1}. The background was determined with a dummy cell, identical except for the

presence of a Pd rod, between days 30 and 33 with a measured rate of $0.1472 \pm 0.0008\ s^{-1}$.

This implies a maximum signal rate of $0.0 \pm 0.0012\ s^{-1}$, which, together with the measured 0.015 efficiency, results in an upper limit of 0.08 n/s for this particular sample. If a D/Pd ratio of one is assumed, then this neutron flux limit corresponds to a (neutron) fusion rate per deuterium pair of < 2.2×10^{-24} (ddn) fusions/dd–pair.sec. In expressing this ratio in terms of a fusion rate per dd–pair, it is implied that the effect (real to the extent of the upper limit) is a volume effect.

IV. Conclusions

In this paper we have reported on the search for neutrons from any possible deuterium–deuterium fusion reactions inside electrochemically deuterated palladium rods. Using a NaI(Tl) scintillation detector we have measured < 0.08 neutrons/sec for a 0.4x7 cm cast, cold rolled, and annealed Pd rod. This corresponds to < 0.007 n/sec.g Pd, compared to PFH's claimed[1] 2.6×10^{3} n/sec.g Pd. Our upper limit corresponds to < 2.2×10^{-24} (ddn) fusions/dd–pair.sec, which is approximately 5x lower than that claimed by Jones' original publication (ref). More recently, Jones has reevaluated his rate to ~ 10^{-24} fusions/dd–pair.sec.[12] Finally, our results are similar in content and precision to those of other researchers reporting "null" results in very similar experiments, with other types of neutron detection systems.[12]

Acknowledgments

We would like to thank T. Boone, R.A. Laudise, D.N. Loiacono, D.W. Murphy, J.T. Plewes, J.W. Rodgers, T.D. Schlabach, B.E. Spear, G.M. Sturchio, S.S. Voris, M.M. Weiss, and G.C. Lindsay (AT&T Nassau Metal

Works) for their various assistance and stimulating discussions and interactions.

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Figure Captions

Fig. 1 Electrochemical cell with a Pd cathode and a Pt anode used in search for neutrons from cold fusion.

Fig. 2 NaI(Tl) scintillator detector system for measuring electrochemically induced dd —> n fusion reactions.

Fig. 3 (a) Pulse height spectrum of a Am–Be calibration source in place of the electrochemical cell measured with a NaI(Tl) detector; (b) typical pulse height spectrum (signal + background) measured during electrochemical deuteration of Pd rods. The various labeled lines result from natural radio activity background with the energy in keV and the parent of the corresponding decay chain.

Fig. 4 Spectrum used in the ^{3}He analysis of the deuterium content of a loaded Pd rod. The spectrum indicates Rutherford back scattering from the Pd and proton and alpha particle signals from ^{3}He–d interactions. The energy associated with the outgoing proton (1.17 MeV) is determined by the energy loss in the solid state detector with a depletion layer smaller than the proton range in silicon.

Fig. 5 Counting rate in the 2.2 MeV region showing the vulnerability of this spectral region to natural radioactivity background. The spike occurred by temporarily disturbing the shielding during days 16 and 17 after the start of the electrolysis process.

Fig. 6 Counting rate in the 3.5–7 MeV region during the electrochemical deuterium loading of 3 Pd rods. Data between days 17 and 33 are from a single cell with a 0.4 cm diameter Pd rod. Background data (with identical cell, but without the Pd rod) are shown between days 30 and 33.

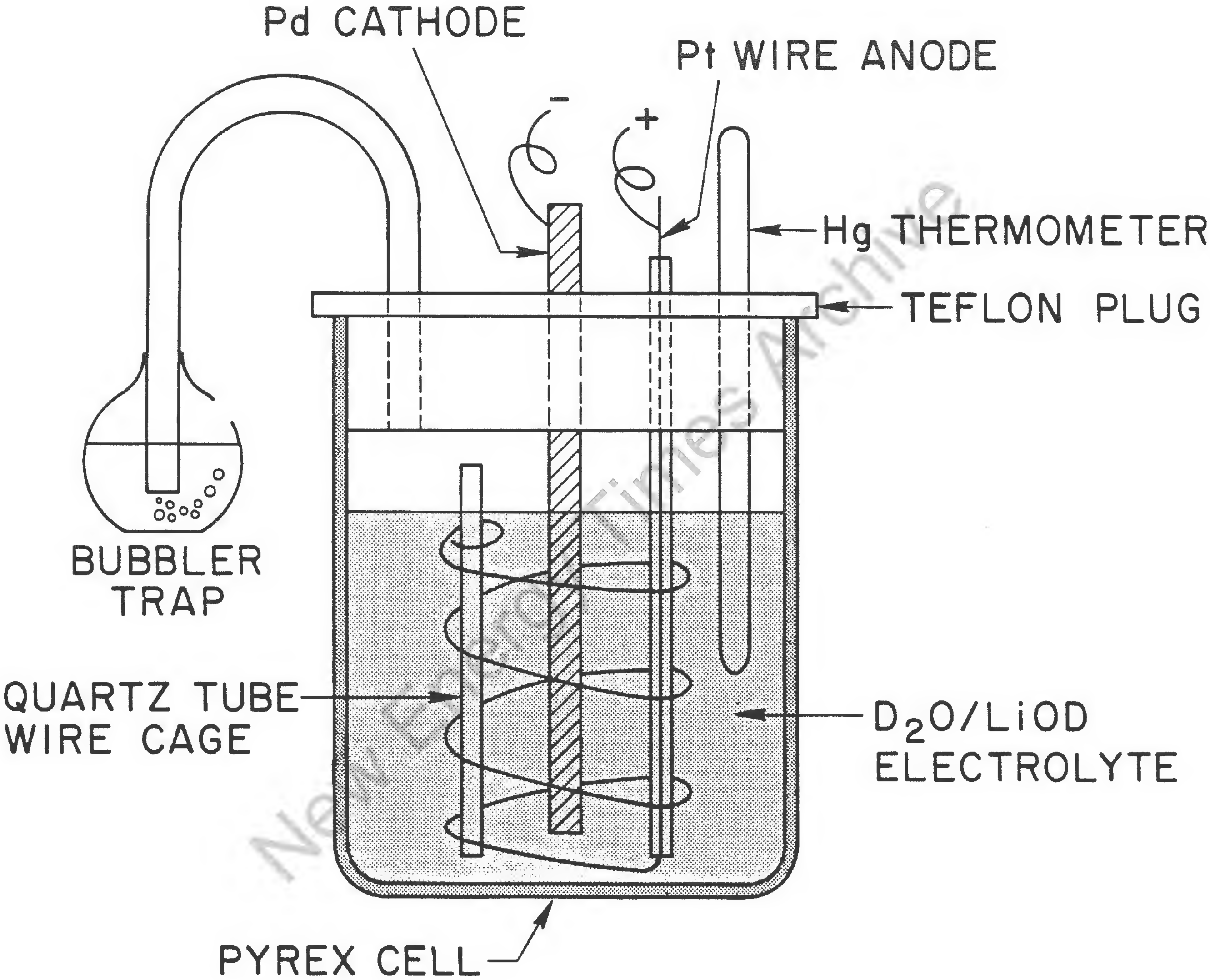

Pd CATHODE
Pt WIRE ANODE
Hg THERMOMETER
TEFLON PLUG
BUBBLER TRAP
QUARTZ TUBE WIRE CAGE
D_2O/LiOD ELECTROLYTE
PYREX CELL

Fig. 1

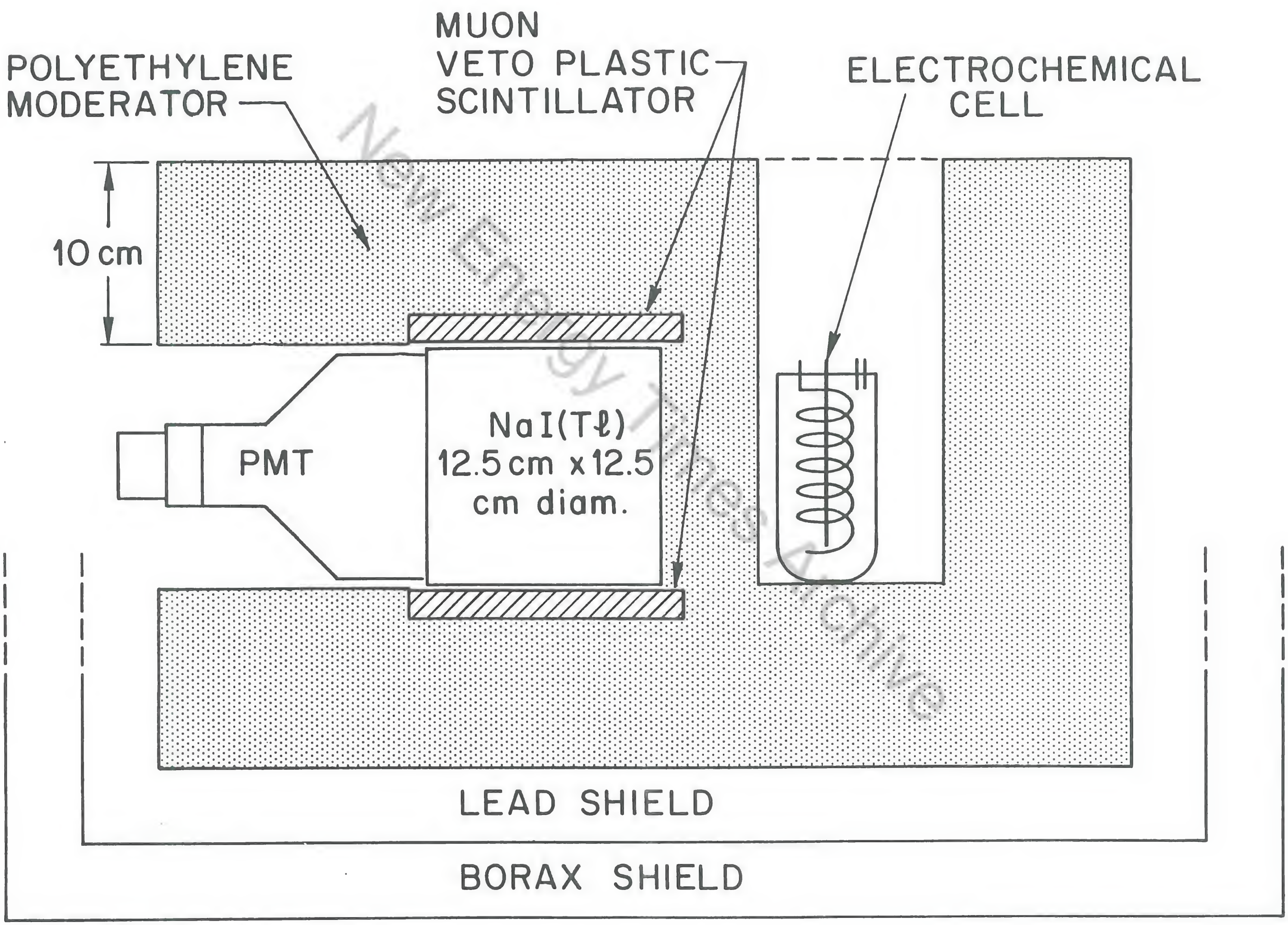
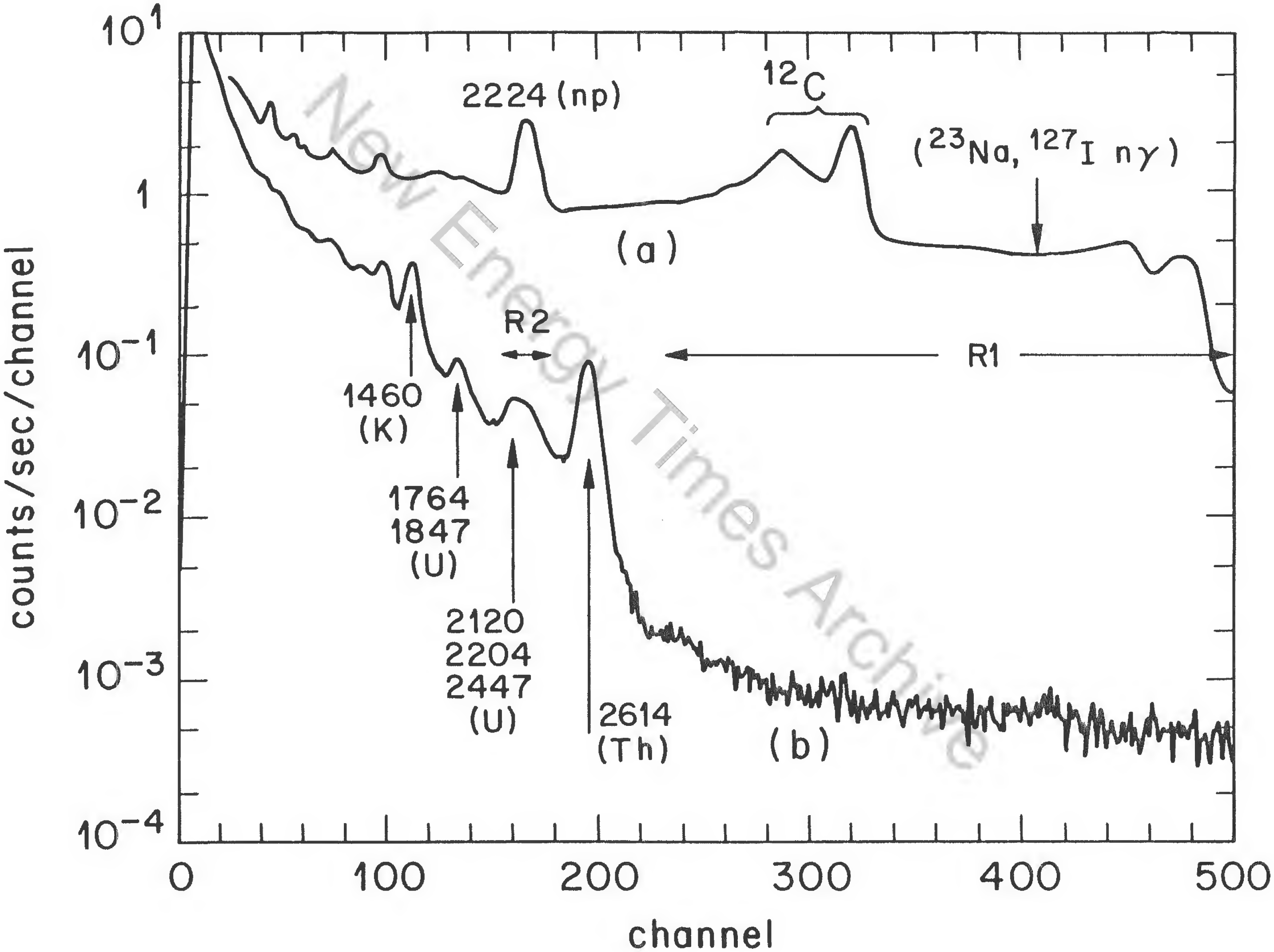
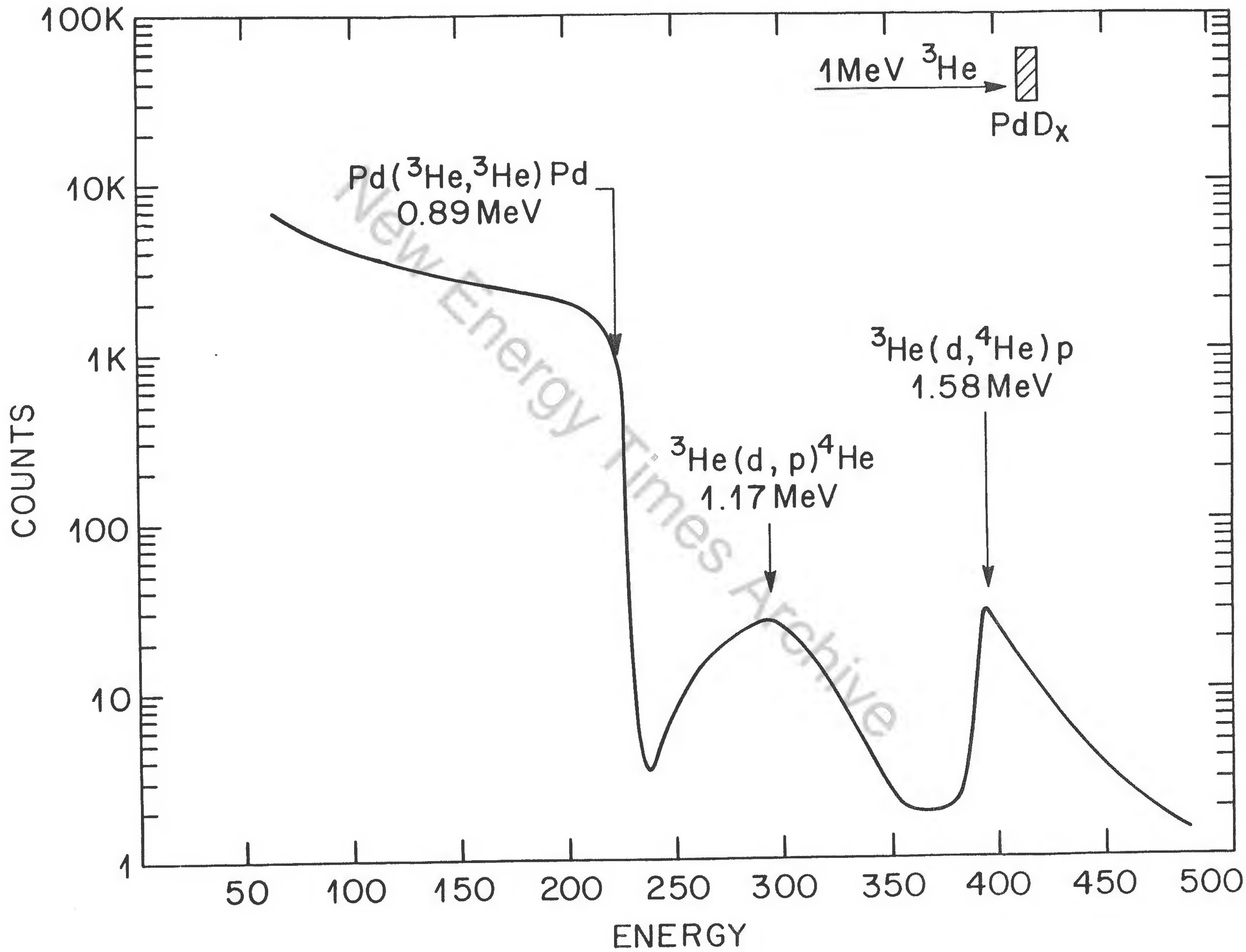

POLYETHYLENE
MODERATOR
MUON
VETO PLASTIC
SCINTILLATOR
ELECTROCHEMICAL
CELL
10 cm
PMT
NaI(Tℓ)
12.5 cm x 12.5
cm diam.
LEAD SHIELD
BORAX SHIELD

Fig. 2



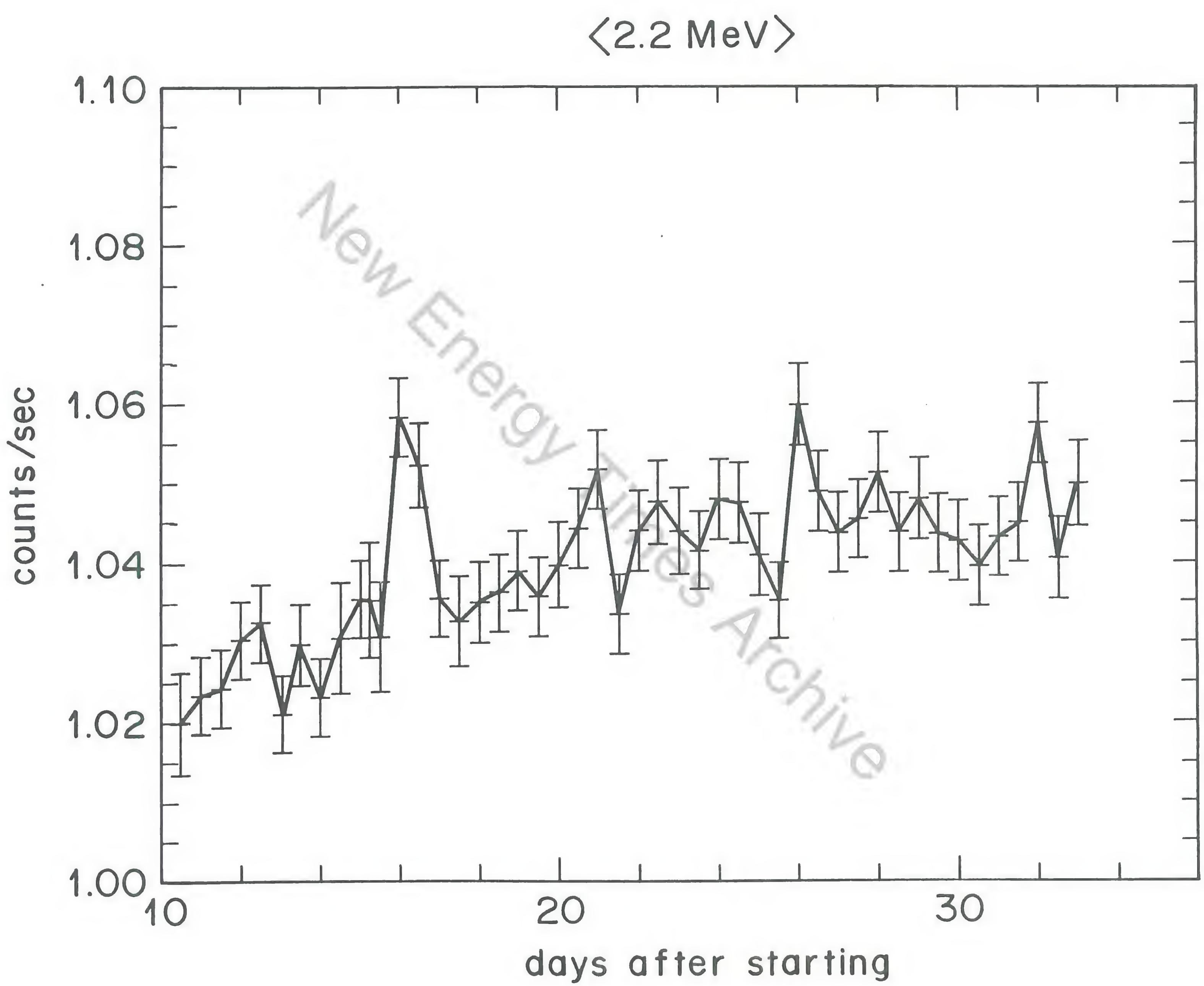
10^1
1
10^-1
10^-2
10^-3
10^-4
counts/sec/channel
0
100
200
300
400
500
channel
2224 (np)
12C
(23Na, 127I nγ)
(a)
(b)
R1
R2
1460
(K)
1764
1847
(U)
2120
2204
2447
(U)
2614
(Th)

Fig. 3



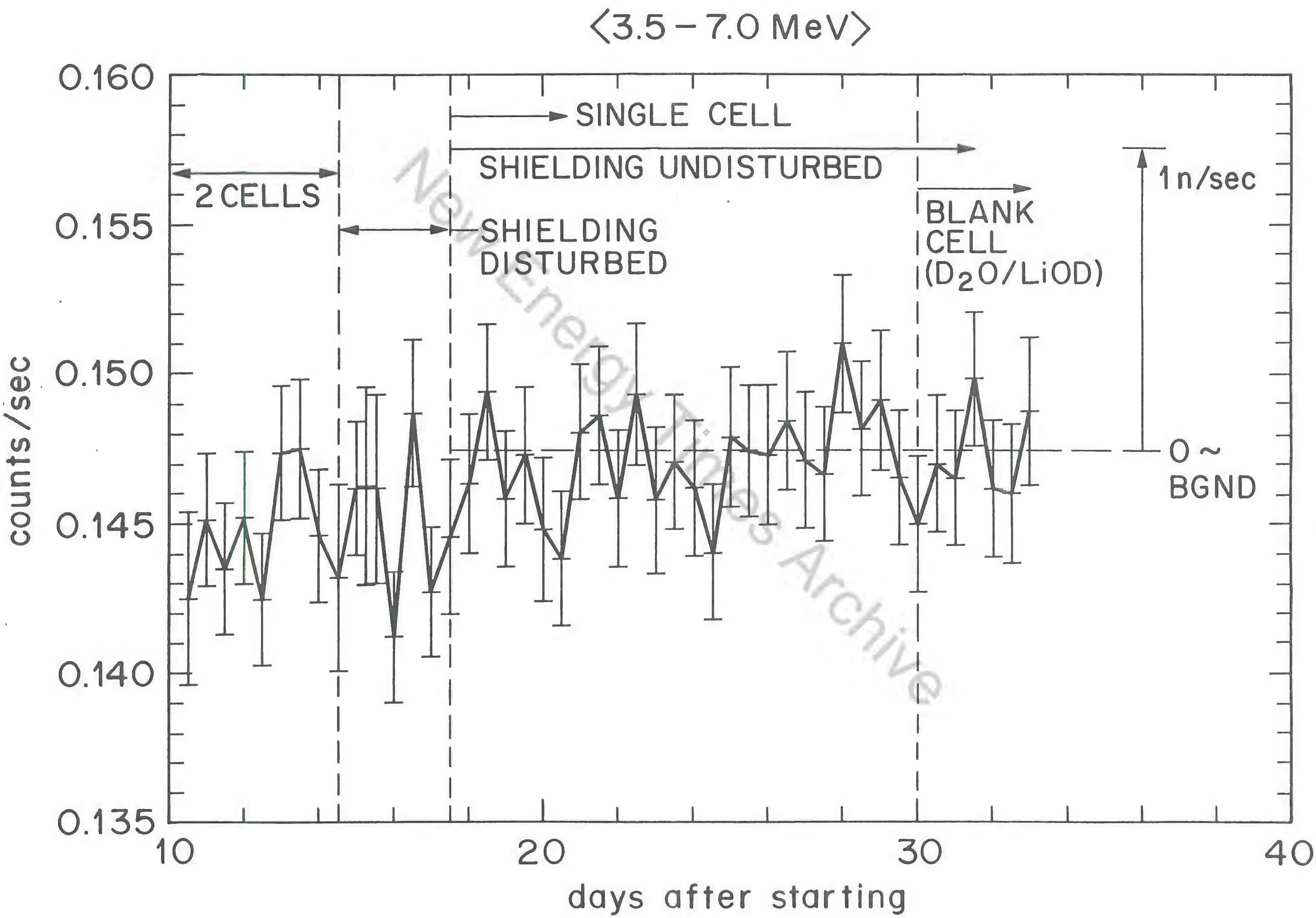
100K
10K
1K
100
10
1
COUNTS
50
100
150
200
250
300
350
400
450
500
ENERGY
1MeV 3He
PdDx
Pd(3He,3He)Pd
0.89 MeV
3He(d, p)4He
1.17 MeV
3He(d,4He)p
1.58 MeV

Fig. 4



⟨2.2 MeV⟩
1.10
1.08
1.06
1.04
1.02
1.00
counts/sec
10
20
30
days after starting

Fig. 5



<3.5 - 7.0 MeV>
0.160
0.155
0.150
0.145
0.140
0.135
counts/sec
10
20
30
40
days after starting
2 CELLS
SINGLE CELL
SHIELDING UNDISTURBED
SHIELDING DISTURBED
BLANK CELL (D_2O/LiOD)
1 n/sec
0 ~ BGND

Fig. 6

TEXAS A&M UNIVERSITY

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July 3, 1989

Professor Norman Ramsey
Physics Department
Harvard University
Cambridge, MA

Dear Professor Ramsey,

I write to to tell you that on Friday June 30 my graduate student Nigel Packham (Imperial College) examined the tritium content of the gases being evolved from one of our Pd electrodes which had been giving D_2 for a considerable time, the anode being nickel.

We ran the gases over some "recombinination pellets", kindly provided by Dennis Corrigan of General Motors. The resultant liquid gave about 10^8 counts $(\text{min ml})^{-1}$, of T. The measurment instrument was a Wallac 1410. No chemiluminessence was measured in the samples.

This result is of seminal importance and it goes along with the earlier results, in which we had reported finding T (confirmed in four external laboratories) in the liquid and conclusively proves that a fusion reaction is occuring under our conditions at our Pd electrodes.

So called Cold Fusion appears to be a phenomenon occuring <u>on the surface of the electrode</u> for the effects switch on and off depending on an uncontrolled state of the surface and there seems to be no T <u>inside</u> the Pd Thus, Ed Storms (Los Alamos) immersed an electrode which had been producing T in a virgin solution of LiOD and made the electrode anodic (i.e. dissolving from inside instead of cathodic, - all evolving). No new T was produced.

A new field ("Nuclear Electrochemistry") has thus been born. Very much remains before us:

(1) We need to reduce the sporadicity of occurrence of these phenomena. Just what surface species provoke it and how do they build up? They are unlikely to consist of palladium!

(2) We need to confirm that the heat does occur in an amount proportional to the surface, not the volume; and that's its production is coupled to that of the tritium production.

(3) We need to find out what determines the degree of heat output per unit area.

The new insight that the phenomena are surface-based serves to interpret the adventitious nature of the observations and increases the possibility that we shall be able to use of cheaper electrode materials.

Sincerely,

J. O'M. Bockris

COLD FUSION: SEARCH IN DEUTERATED PALLADIUM AT TEMPERATURES FROM 4.3 TO 400 K AND PRESSURES TO 105 kBAR

Isaac F. Silvera and F. Moshary
Lyman Laboratory of Physics
Harvard University, Cambridge, MA 02138

We have studied a sample of deuterated palladium at pressures up to 105 kbar and temperatures from 4.3 to 400 K in an effort to detect cold fusion. A D/Pd concentration of 1.34±0.1 was achieved. No evidence of cold fusion was found with a limit of 2.5×10^{-18} fusions/DD pair/s from neutron detection and 1.6×10^{-8} fusions/DD pair/s from detection of possible heating; a gamma ray detector showed no evidence of gamma rays differing from background.

Bockris

DEPARTMENT OF CHEMISTRY
TEXAS A&M UNIVERSITY
COLLEGE STATION, TEXAS 77843-3255

MEMO

R GARWIN
(IBM)

We have correspondence
JA

DATE: 6/12/89

TO: "COLD FUSION" VISITORS

FROM: JOMB

In the following, we seek to avoid misunderstandings about the work on electrochemical Cold Fusion which is going on (in three sub-groups) at Texas A&M University.

1) We are interested in the experiments reported by Fleischmann and Pons, and by Jones, which mention cold fusion obtained by electrochemical confinement.

2) We take the attitude that the presence of cold fusion in the experiments carried out by these workers is unproven.

3) Our attitude is to stress experiment. We seek to find out whether there <u>are</u> neutrons evolved from palladium electrodes under certain circumstances; whether tritium <u>is</u> produced during deuterium evolution at palladium electrodes; and whether the sometimes observed excess heat can be replicated in our laboratories.

Of course, we are interested in attempting to bring the reproducibility under better control.

4) When we have obtained reproducibility in the region of > 50%, and can instruct others how to do the experiments with the same success rate, then we shall investigate the dependence of heat evolution, neutron production and tritium evolution as a function of the variables such as overpotential, metal substrate, D/Pd ratio, dislocation density, dendritic promontories, etc.

When we have established some of these dependencies, perhaps in a year or

so, we shall then have a basis on which to decide if the New Phenomena originate in nuclear processes.

5) We are particularly unenthusiastic in the discussion of the application of present theories of fusion in plasmas to idea of fusion in electrochemical confinement because we think that the difference of conditions, particularly in respect to screening by electrons of deuterium-deuterium interaction, is an extreme one, and that it has not yet been properly investigated theoretically.

Our attitude is that we _may_ be in an emerging area of science, and that in such situations experiment usually molds theory to fit it.

Historically, when new science is emerging, it is often reviled and denigrated until the new paradigm is accepted. It is, of course, too early to say whether this is the situation in this field.

6) At the time of writing, the phenomenon is less than three months old. Two or three years (5-6 Centers, 100 people) will be the right sort of time to think of in order to make a decision as to whether it is worth Big Money. The idea that a number of meetings are already planned, and even decisions made up on the basis of happenings at them at this time, appears to us to be unwise, partly because of the emotional outbursts by physicists which have occurred at some of them and the great negativity widely shown; but mainly because of the small degree of knowledge among us all.

Although we welcome criticism, we believe that spending a great deal of time in angry condemnation of the phenomena we are investigating is not a good way to further understanding of New Phenomena which understandablt exist. We would rather tell you in a relaxed way, about our results, and compare them with the positive results of others in various parts of the world. We believe it is _agreement_ among scientists, particularly between those in various

countries, which eventually decides what is regarded as "truth" for a few decades in a field.

We think the new (and shaky) "facts" should be isolated from comparison with the older theories until the facts are firm and agreed upon - at least to a good degree.

8) About negative results: We think that, in attempts to verify a newly claimed phenomena, negative results have much less value than positive ones. Negative results can be obtained without skill and experience.

It has always been the anomalies which can be seen in a Science which gives rise to the new ways of thinking which cyclically invade the sciences. The constant reiteration of the old way (particularly with the great Anger and Emotion) we are seeing among our colleagues and visitors has not been the way that changes in scientific attitudes have come in the past.

Therefore, when persons tell us that they have carried out the electrolysis of deuterium evolution in palladium and see nothing new, particularly if (as is usual) they are furious about it, have spent little time on it, and have little experience as to how to do experiments of the type named, we tend to discount their contribution.

This is particularly so because the phenomena under consideration are undoubtedly elusive. Added to this is the fact that the effects - when they indeed turn on - are difficult to find in electrodes as small as 1- and 2-mm diameters (quickly chargeable), and can only easily be detected (when they display) in most calorimeters when the size of the electrode is something in the region of 4-6 mm. However, a 6-mm electrode takes 72 days to charge before the experiment can begin.

Thus, as we are now less than 72 days from the announcement, and as to start experiments it will be necessary not only to charge electrodes but to

gather equipment of various kinds both electrochemical and nuclear, -to say nothing of super-pure Pd rods, - it is remarkable that those who were not already working in electrochemistry before the announcement was made could have made experiments at all, let alone gotten results upon which the National Policy (in funding) is to be founded.

Most of the experiments in which negative results have been obtained have come from Laboratories which have little record of research in physical electrochemistry; or, when in a tiny number of cases the laboratories were electrochemical, little experience in nuclear measurements.

The most common errors to date are:

a) Insufficient charging times. The latter is obtained from the use of the formula $\Delta^2 = 2DT$. Our habit is to calculate using this formula for α and β Pd (hence, 2 T's), and the D is for the two forms of palladium, then double the time to allow for possibility of a third form which seems to exist. (We get c. 30 days for 4 mms and 72 days for 6).

b) The second most common error is the use of electrodes less than 4 mm in diameter.

There is, at first, an apparent advantage to using these because they charge up more quickly. The disadvantage is that the effects they give are often too small to see: they need a micro-calorimeter.

c) Contact with the wet atmosphere eliminates the observation of the heat. Water must be excluded from the D_2O.

d) Use of inappropriate palladium is difficult to specify. There is some evidence is that Johnson-Matthey's "puratronic" palladium works best, but we do not know why, and the evidence is conflicting. Annealing, hammering, cold work, casting, have all been suggested.

e) The final experiment in which the phenomena are sought should be carried out at more than 500 ma/cm^{-2}. After the charging time of 30 days for the 4-mm rods and 72 days for the 6-mm rods, there has to be a further time up to 500 ma/cm^{-2} which may last several days before anything abnormal is seen.

We do not give up an electrode until 7 days after the current density has been turned up to 500 ma cm^{-2}.

f) Much confusion and waste of money is carried out by examination of electrodes which have never "woken up".

When an electrode doesn't show the heat, there is little point in examining it in great detail with neutron counters ("the most sensitive in the world") or etc., or X-ray monitors. If it does not show heat, it is less likely to produce tritium, etc.

Reports are full of accounts of people who did this and spent time seeking nuclear particles and not finding them (no wonder, if the electrode did not give heat).

g) There has been too much accent on <u>very</u> accurate calorimetry. Our experience is that when the effect switches on, it switches on very definitely, and using the size of rods mentioned above, calorimeters which are only measured to ~100 milliwatts can easily measure the effects.

h) Keeping water out: The separation factor of water to deuterium is 9 times in favor of water, so that a 0.5% water-containing solution will evolve about 5% hydrogen. Small amounts of hydrogen seem to poison the electrode.

It's probably better to keep the water below 0.1%.

i) The use of LiOH instead of LiOD: Remarkably, a number of laboratories have used LiOH - not good for obvious reasons.

j) Lack of preelectrolysis of the solution: The latter is very

necessary, for it removes the water and takes away other undesirable impurities.

k) Lack of knowledge of the Tafel parameters

Exchange current density?

Overpotential?

D/Pd ratio?

We have found that these vital basic elements are understandably little known to physicists working on fusion. It's vital to know them because the fugacity developed in the electrode depends upon the detailed relationship between the Tafel slope and the overpotential, - and then depends on the relevant mechanism of deuterium evolution, intermediate concentration, etc.

l) Neutron measurements: Arrangements for screening out cosmic ray showers are, of course, essential. Neutrons as a function of the <u>state of the surface</u> are informative.

m) Tritium: Tests for the elimination of chemiluminescence is essential. The plot of the tritium build up in the solution as a function of time may be informative. Conversely, we don't always find tritium when we find heat.

Finally, there is no doubt that irreproducability is the bane of these experiments. We are looking increasingly towards the concept that the phenomenon occus at the surface rather than in the interior, although of course the state of internal saturation will effect the surface concentration of intermediate deuterons.

FOR ERAB Panel 07/11/89 ~~07/09/89 ? Happer, Koonin, Nelson from Dick Garwin~~

Received: from CEARN by CEARN.cern.ch (Mailer R2.03B) with BSMTP id 6139; Sun, 09 Jul 89 12:46:38 GVA
Received: from mint.cern by CEARN.cern.ch (IBM VM SMTP R1.2.1) with TCP; Thu, 07 Sep 89 12:46:24 GVA
Received: from cernvax.uucp (cernvax) by mint.cern (cernvax) (4.12/3.14) id AA22578; Sun, 9 Jul 89 12:45:57 +0200
Received: by cernvax.uucp (5.57/Ultrix2.0-B) id AA01259; Sun, 9 Jul 89 12:37:13 +0200
Message-Id: <~~8907091037.AA01259~~@cernvax.uucp>
Date: Sun, 9 Jul 89 12:36 GMT +1 By D.R.O. Morrison.
From: ~~MORRISON%VXPRIX~~.decnet.cern.ch@cernvax
To: CBOWMAN@LAMPF, JONESSE@BYUVAX, BOND@BNLDAG, ESCOBAR@FNAL, PIRES@BRUSP, gai@yalevm, rlg2@yktvmv, sgrard@bmsueml1
X-Vms-To: @LIST7.DIS

Dear E632 and WA84 Colleagues, CERN, 7 July 1989.

COLD FUSION NEWS No. 16.

1. INTRODUCTION

At the Santa Fe Workshop, Texas claimed large amounts of tritium were observed. Further reports of tritium have come from Los Alamos and then Texas has stated that tritium corresponding to 10 Watts/cm3 of Pd had been found - it might be thought that the consequent flux of neutrons would kill all nearby experimenters, but fortunately they are in good health.

Los Alamos and Steve Jones have submitted for publication a paper describing the observation of neutron bursts, but they have still not performed the most elementary of checks.

Pathological Science is appropriate for discussing mistaken experiments long past and dead, but to discuss living experiments some other Science is necessary where one can study and find discriminating criteria to evaluate whether the result is true or mistaken - in medicine this is called doing a Biopsy. A first approach to Biopsy Science is discussed.

Many other items are reported.

2. NEW RESULTS

2.1 Tritium.

At the Santa Fe meeting, Kevin Wolf reported that his group had observed a very large production of tritium - the exact quantity was not easy to evaluate but it was roughly half way between the 1 E -9 Watts/cm3 Pd for neutrons and the 10 Watts/cm3 Pd for heat of Fleischmann and Pons. It was about 10 orders of magnitude higher than the neutrons claimed by Jones et al. Moshe Gai tried to persuade Dr. Wolf that he was measuring gammas (as Moshe did initially until he quickly checked and found out his mistake), but did not succeed.

Last week I received a brief telegram "Tritium confirmed by Los Alamos " from Prof. Bockris. On enquiry I found a report that 4000 counts per ml had been obtained by Edward Storms and Carol Talcott, but the time interval was not given, though the authors said they had significant results. The situation was described by the Los Alamos spokesman, Jeff Schwartz, "The bottom line is that we've got something that maybe is interesting, but it may not be interesting also. The work has to be be reproduced, confirmed, peer reviewed and published". This makes the situation much clearer to those who read between lines.

On Monday, while in a WA21 Collaboration meeting and one hour before I was due to give a lecture about Cold Fusion and Biopsy Science, I received a very long telegram from Prof. Bockris describing their measurements of tritium. He was justifably enthusiastic as the yield of tritium was very high - "8 electrodes out of 12 measured gave counts per min. per millilitre in solution of 1 E 5 after high current electrolysis 10 hours." "If assumed that measured T represents DT and that DT in solution is in equilibrium amouunts at end of S shaped buildup correspond (are about 40% greater than) ten Watts per cc from hypothesized D + D ---> T + H. Hypothesis is that this formed on surface as no tritium found inside electrode". All these tritium results justify Prof Bockris's enthusiasm and he is quoted in the Deseret News as describing them as "of immense world-influencing importance". At my lecture I showed an enlarged copy of the telegram and faithfully read it out. I pointed out that there were two reactions,

d + d ---> t + p (1)

and d + d ---> 3He + n (2)

From all that is known, these reactions are almost equal in cross section, to better than a factor of two. Introducing spin factors has been suggested to alter this at some energy but a factor of a billion is unthinkable. Hence if 10 Watts equivalent of tritium were observed, the flux of neutrons should be enormous and rapidly fatal.

In replying to Prof. Bockris, after pointing this out (and trusting he was in good health), it was suggested that if it were assumed (wrongly) that reaction (2) was suppressed by a factor of more than ten orders of magnitude, then there was still the problem that the protons produced in reaction (1) would have 3 MeV. And 3 Mev protons would produce neutrons by charge exchange

p + n ---> n + p (3).

These neutrons would be almost as frequent as those from reaction (2) and again would give a fatal dose of radiation.

In his latest telegram Prof Bockris said they are now getting 10 to the eighth counts per unit from the collected gases which were converted to liquid!

As Texas A&M is still active, it must be assumed that as Dick Garwin said, "They are counting something, but I do not know what it is". Certainly it cannot be tritium from fusion at those levels.

2.2 Neutron Bursts

At the Santa Fe Workshop, Dr. Menlove presented a paper on the 24 May describing the emission of bursts of neutrons from a cylinder containing D2 gas and Titanium about 40 to 80 minutes after the cylinder was warmed up from liquid nitrogen temperatures. The results were spectacular and effects of up to 11 standard deviations were claimed. As many results had suggested that effects were erratic, this result was very welcome. I asked the first question, "Have you repeated the experiment with normal hydrogen gas?". I was told the experiment was very recent and there had not been time to do it before the Workshop but they would do it next. I reminded them of the controversy with Nature on this subject. So was very surprised to receive a preprint, submitted to Nature, containing these results plus many more, but still the most elementary test, repeating with normal hydrogen had not been done. As a form of control, they had used an inert cathode, but this is not

Wrong. Presumably he means p+d → ~~dd~~ p+p+n. But this is limited by proton range and should be a 10^{-4} effect. Still, one should see plenty of neutrons from it!

enough if one wishes to know if fusion has occurred. With this important reserve, will comment on the rest of the paper.

Various forms of Pd and Ti plus Ti coated with Pd were used. Turnings, sponge, foils, crystals and powders were used [this might suggest that the details of the surface is not too important - my comments are in square brackets]. The time spread, of the individual bursts was less than 100 microseconds. In addition to these early bursts, they also observed "random bursts" occuring up to 30 hours after warm-up. They also used Jones type cells with electrolysis with cathodes of Ti, Pd, V metal;, foils, crystals, and sponge. The results were erratic, some neutron emission being observed - they are now trying to evaluate which conditions corresponded to neutron emission.

An important point about the emission was that the counting rate was very low, 0.05 to 0.2 neutrons per second for random emissions. It was only possible to find it by requiring the (3He) counter tubes to give time coincidences, the gate being 128 microseconds [wonder if they look at the rate as a funtion of time to avoid having a peak in the first bin]. Moshe Gai says the background counting rate is exceptionally high, 900 to 1400 counts per min to be compared with 80 to 100 cpm at Cal Tech and 1 cpm for him with comparable detectors. In addition he says the room is exceptionally well shielded. He writes "Why is Cold Fusion only observed in noisy experiments?" It is intended that Drs. Menlove and Jones will go to Moshe's lab to repeat their experiments.

At Santa Fe, Dick Garwin had talked about fractures in the D-loaded cathode where sudden fractures should be heard by using a microphone and he recommended groups to do this. He also mentioned that such fractures could give off ions. Another speaker tried to suggest that intense electric fields produced in fractures could induce fusion, but quick detailed calculations suggested that this should not occur. Now in the Menlove et al. paper several references are given to the "possibility of particle acceleration during a fracturing process" and they quote "both electron and positive particle emission has been observed at energies of several KeV". Menlove et al. say that "fracturing and fatigue" have been observed. So far they have not been able to establish any correlation between neutron emission and fracturing but are studying further. [Thus there are strong indications that the very low level neutron bursts they claim are not due to fusion but may be a consequence of fracturing of the metal when it is stressed by loading with deuterium and by temperature changes.]

Another interesting question is whether they do confirm the level of neutron emission observed by Jones et al. originally. A clue is that they say that their highest random emission yields are similar to Jones et al. which must mean that their average yield is much less?

2.3 Second Closed Calorimeter result

Dr. Kreysa of Frankfurt who will shortly be publishing their interesting paper, told me they are also looking at fracturing. When I said it was a pity that only one group had done the ultimate calorimetric experiment with a closed calorimeter, he mentioned that Dr. Schmeider of Karlsruhe had done it and found a null result. Am still trying to contact him for further details.

3. RE-ANALYSIS OF ORIGINAL CLAIMS

3.1 Gammas Claimed by Fleischmann and Pons.

Petrasso et al. from MIT had published a letter in Nature 339 (1989) 183 [1] in which they pointed out inconsistencies in the gamma ray spectrum claimed by Fleishmann et al. in their paper in J. Electroanalyt. Chem. 261 (1989) 301 [2]. In the 29 June issue of Nature, Fleischmann, Pons, Hawkins and Hoffman reply to this and Petrasso et al. reply.

Fleischmann et al. start by claiming that Petrasso et al. [1] used a spectrum shown during a television broadcast and this was wrong! In their reply, Petrasso et al. state that they used the published spectrum in ref [2].

Fleischmann et al. show a much more complete spectrum than in their paper and it includes peaks at 2.61 and 1.46 MeV from 208Tl and 40K. They find a peak at 2.496 MeV only with the counter near the cell not with the counter 5 m from the cell (their background) [a very poor way of finding background]. They say the interpretation of this 2.496 MeV peak is in doubt.

Petrasso et al. make the following points;

1. Fleischmann et al. do not reply to their criticism that the published peak at the expected value of 2.2 MeV is a factor two too narrow for the well-known characteristics of their counter
2. Fleischmann et al. do not reply to their criticism that the peak lacks the rising Compton edge that should be visible on their plot at 1.99 MeV
3. They give no explanation how the published peak at 2.2 MeV has become a peak at 2.496 MeV
4. Petrasso et al. make an analysis of all the peaks in the Fleischmann et al. letter, establish their energy and note that several are "instrumental artefacts".

This is an unhappy story. Fleischmann et al. have not improved their credibility with their letter to Nature. I only found this the day after talking to Martin Fleischmann and so could not ask him about it.

3.2 Neutrons Claimed by Jones et al.

In the original paper by Jones et al. Nature 338(1989)737, neutron emission was claimed on the basis of a small peak above a normalised background which they claimed was five standard deviations. When Steve Jones gave a lecture at CERN on April 17, he gave some numbers to explain how 5 std. dev was obtained for Run No. 6 (these are given in Cold Fusion News No. 10). Yves Declais, Charles Peyrou and others said that the errors coming from the scaling of the background should be included and this would reduce the significance. Now I could not understand this being important as the statistics looked large in the figure. However I was shocked to find in the figure caption that 10 counts had been added to each bin "for clarity of presentation". Thus 10 counts meant zero and 11 meant one count. I have never seen counts invented to make a graph look better and I hope never to see it again.

After Steve's talk at the APS on May 1st, I repeated this and said that I had calculated the significance on the basis of his numbers and obtained 2.2 std. dev. which is what the peak in the graph looks like. Later Steve talked to me at length and finally agreed that they should recalculate including the error from scaling the background. However at the Santa Fe workshop on 24 May, the same graph was shown and five standard deviations claimed.

Nature is now going to publish a letter from Moshe Gai about the Jones et al. paper and also a reply from Jones et al. This reply nows gives the results plotted without any invented counts and looks very nice and looks to justify the claim of a four standard deviation effect.

What the letter does not discuss is that there are other experiments carried out carefully which give appreciable lower neutron rates by one or more orders of magnitudes (AT&T, Yale-BNL, Bugey, and Cal Tech). Hence the overall conclusion weighing all the results, is that their is no evidence for neutron emission from cells. [An interesting argument put forward was that the results of Yale-BNL did not count because of all their possible combinations of different cathodes and different electrolytes, only one corresponded to the Jones et al. work - is one not enough if the upper limit is a factor of a hundred lower?].

4. NEED FOR BIOPSY SCIENCE

In a magnificent lecture in 1953 which was the fruit of long experience, Irving Langmuir described three famous experiments that were mistaken - N-Rays, the Davis-Barnes effect and Mitogenetic rays. from these he derived 6 characteristic symptoms of Pathological Science. He then confirmed them by considering the Allison effect and Extrasensory Perception. From early experiences that I witnessed with mistaken results, I was inspired to study the subject further and in 1976 to extend it to the Problems of a Scientist in a Non-scientific World - considering things such as Supernature, the Loch Ness Monster and Uli Geller. Also I extended the number of characteristic symptoms to 12. This has an advantage in discriminating between correct and mistaken results thus;

with Langmuir's 6 symptoms, wrong results scored 3, 4 or 5 while correct results scored 0, 1 or 2

with 12 symptoms, wrong results scored 6 to 8 while correct results scored 0, 1, or 2 giving more discrimination

(another difference is that I changed Langmuir's time chronology to the

present Phase 1 (original and confirmations only), Phase 2 (half yes and half no), and Phase 3 (avalanche of Noes).

Pathological Science was developed for experimental effects many years in the past (and this is safer to save the proponent from irate believers). When I talk of Pathological Science to believers in Cold Fusion, they get very unhappy as they consider pathology to be only for the dead. When a person is alive but has a suspected problem, the doctors perform a Biopsy, i.e. they make an examination to see if the effect is healthy or not. So there is the need for Biopsy Science which will study living experiments.

Cold Fusion has the special feature that the dream of abundant energy with little pollution is so powerful that it is important to be able to develop criteria that allows one to distinguish whether the claim was correct or not. And to do so quickly while experiments are still being performed. It is felt by some that Cold Fusion is doing great harm to Science, much more than any benefits it may bring.

Hence have tried to develop a set of Symptoms which would discriminate as well as possible between correct and mistaken effects. Have done this in 2 ways;

1. Extended the number of characteristics of mistaken results to 15
2. As some characteristics are more important and are objective rather than subjective, have introduced weights for three of them;
   a) weight 10 - If results are not reproducible
   b) weight 3 - If results show regionalisation, i.e. in some regions of the world almost all results Yes and in other regions almost all results are null
   c) weight 3 - If the time dependence in a region is Phase 1, Phase 2, then Phase 3

A preliminary presentation was made at the lecture last Monday. Correct experiments quoted had scores of 0, 1 or 2. Mistaken effects scored from 6 to more than 15 for N-Rays. The Fleischmann - Pons effect of excess heat scored 25 and the fusion product type results scored 22. High temperature super-conductors score 2 while room temperature super-conductors score 12.

Now an obvious criticism of this is that it is partly subjective and that in time experimental results will decide. A normal comment but there are two important arguments this neglects;

1. It takes an incredibly long time to convince people who have a dream that experiments are destroying their dream. For example all the good careful experiments that have been performed on cold fusion have given null results (does anyone know of a good careful experiment that has made all the checks and has good reproducible data that are available and which gives a positive result?). At this time most scientists still feel that maybe there is something there - it seems impossible that all the 40 or so experiments giving positive results could be wrong. (when Houdini was exposing mediums, it was generally not too difficult for him - except in the case of the Donovan brothers where nobody could accept that the fake effects were being produced by as many as 11 people who appeared in different combinations of 2 or 3). Thus there is an additional need for some criteria to recognise mistaken effects early.
2. To recognise mistaken effects quickly, it is necessary to have a good historical and philosophical background. If you have studied a few case histories, it is much easier to recognise a wrong result. At present people say "it smells wrong". But if one applies Biopsy Science, it is possible to give a number to an effect and see how it fits on a plot of true and wrong effects.

It takes some study to evaluate an experiment to get its Biopsy value. At present I would like to study the above proposals further before writing anything to see if some improvements can be be made.

5. DISCUSSIONS

On Wednesday morning spent an hour describing Cold Fusion, Pathological Science and Biopsy Science with Andrei Sakharov. In particular we discussed the tritium results - he has some experience in these reactions. He seems to have appreciated it as am told he spent much of the evening explaining it all to a colleague. When I was talking of how in 1957 some British scientist working at Harwell on Zeta persuaded Sir John Cockcroft that the neutrons they had observed meant that fusion had been produced and Sir John then gave a press conference to announce fusion, Andrei

thought I was talking about an incident that happened in Russia also in 1957. Again some scientists found neutrons and told their leaders they had found fusion. When it was discussed at a general meeting, Andrei was young and waited for his seniors to speak. The two most senior people said that if it were true then by doubling the current in the pinch, many time more neutrons should be produced and they should test their hypothesis experimentally. They found only a small increase and so looked for other explanations and realised that the intense field at the pinch accelerated particles which knocked on neutrons, so no fusion. He said that they had not heard of the Harwell effect - which is perhaps surprising as it was spread world-wide as is usual with claims of fusion. Had not heard the Russian story before.

Then I asked his opinion about going to China as have been very busy as the long term secretary of a series of conferences which are scheduled to have a meeting in China in September. David Gross had come in and he had been in Beijing in the early days of June at a conference that was advised to end early so he gave us an account of what he had observed.

In the afternoon went to Lausanne for a long talk with Martin Fleischmann taking along a big file of results. We agreed that closed calorimeters were best though he said he would have preferred that the British Columbia one did not use a rubber bung. We discussed the Harwell series of experiments which I regard as the most complete (for possible reason see above!). He said he wished to wait and see the published results so as not to be open to accusation of biassing them - I warned him that David expects to publish in about two weeks. He said they were planning to work on other calorimeters in Utah.

6 OTHER ITEMS

6.1 Poland

When I was in Poland last week at a E632/WA59 Collaboration meeting, was told that 8 groups had performed experiments, 5 finding null results and 3 positive results - one of these positive results (by a military-related organisation) was still believed in (later was told they had been given some of these notes). At the end of my lecture on Cold Fusion, Prof. Kreft described the work of their Krakow group using Pd and Ti. No effects were found and upper limits for a Pd and a Ti sheet of 1 E-22 and 2 E-23 fusions/d pair/second were found at the 10 standard deviation level.

Another group said that they had done an experiment, found a null result, and would not publish.

6.2 Brazil

Have heard that in Brazil, after the 23 and 24 March press conferences, the work of the Brazilain group on a tokamak was interupted and the 40 people started to work on cold fusion. Initially they found some effects but after two months went back to hot fusion.

6.3 Progress of the DOE Panel

The panel has now visited Texas on June 19, (that is before the recent claims of abundant tritium production) and Cal Tech on June 20. After a first working meeting they should have a meeting on July 11 and 12 and it is hoped to issue a first report soon which is much earlier than their original deadline of the end of August.

6.4 Funding in Utah.

Immediately after the first press conferences, $5 million was set aside for fusion research provided that the results were confirmed. Now according to the Deseret News of 26 June, Governor Norm Bangerten has said that U. of U's reseach has been confirmed enough for the $5 million in tax money to be released to turn the scientific experiments into money-making energy producing machines.

The latest U. of U Review is mainly about cold fusion and says that an extra $400 000 has been given as a funding supplement by the ONR. Also Prof. Pons has been given a anonymous private gift of $100 000 [which is what he said he had spent out of his own pocket].

6.5 Research Agreements with the U. of U.

Hugo Rossi has said that they have been contacted by every corporation or foundation involved in energy research for licensing agreements.

Robert Park of the APS says that General Electric has finally signed a cooperative research agreement with the U. of U. involving four GE scientists, one of whom will be located at the U. Johnson Matthey [who have had the Pd cathodes of F&P for a very long time to analyse for fusion products but have not released any results] has signed a letter of intent to negotiate an agreement. The U. says 65 companies have paid for the right to review the patent applications.

6.6 Other News from Utah

Commenting on the report that Harwell has carried out a very extensive series of measurements and have established very low upper limits, Dr. Brophy, the Vice-President for Research commented in the Alburqueque Journal that "it is disappointing that they have not been able to do the experiment properly".

When the State wanted to engage a lawyer to handle patent applications, they found that most lawyers in this field were already handling applications. Security in lawyers offices is now being tightened after a bug was found in a telefax.

6.7 Cuts in Funding of Hot Fusion

Robert Parks of APS said that the President's budget called for $349m but the House Energy and Water Appropriations Committee cut $69m from it but $25m was restored by the House. It is hoped that the Senate will restore the rest. The Committee also refused construction funds for the new Compact Ignition Tokamak and for the Confinement Physics Research Facility. [It is not clear if work on cold fusion had an effect or whether this was an expression of discontent with the disagreement among research groups as suggested by The Scientist].

6.8 Montreux Conference Postponed

Nature of 25 May contained an advertisement for "Fusion '89 ", the 1st International Conference on Cold and Hot Fusion to be held 6 to 8 November 1989 in Montreux, Switzerland. This Conference has now been postponed and no future date set.

6.9 Price of Palladium.

Prof. Kreysa has sent me some nice graphs of the evolution of the prices of gold, silver, platinium and palladium. The general trend of the first three is to decline over the past year. Palladium fell to below $120 per troy ounce in September then rose since with a sharp rise on about 24 March from $145 to $180 early in April and now it has steadied at about $150. The increase from September onwards probably had nothing to do with cold fusion but was following an announcement that for catalysts for car exhausts, palladium could replace platinium which is much more expensive at over $500 per troy ounce.

6.10 Excess Heat Claimed from Titanium

In a previous note, I had commented that all the claims of excess heat were for palladium. Now have found a paper from the Tata Institute of Bombay, Indian J. of Technology 27 (1989) 175, where excess heat from Titanium is claimed.

Douglas R. O. Morrison.

PS At the Lunch for Andrei Davidovitch everyone seemed to be experts on String theory except us. From Gabriele Veneziano I learnt an interesting thing - that when one quantisises string theory, then it is possible to include General Relativity. However learnt that this exciting theoretical result is contested - though Gabriele's explanations seemed reasonable to me though I am not a good judge.

PPS Although this is intended as a private series of notes about physics for colleagues, don't you sometimes get the strange feeling that someone is listening in?

Barry Miller
4/27/89

Texas A & M Calorimetry

1. Appleby / Srinivasan	Heat Flux / Tronac 350
2. Martin	Heat Transfer / Substitution Method
3. Bockris	Heat Transfer / Power - ΔT Calibration

COMMON FEATURE:
All cells are vented.
Recombination of D_2 / O_2 is assumed not to occur:
(V - 1.54)

Heat Flux Microcalorimetry

Depends on efficient heat transfer from electrolyte cell through surrounding thermoelectric device array into heat sink.

Practical: 3μW - 3W range.

At 600 mA/cm^2, typical input powers are ~ 500 mW.

Constant current supply ~ 100mA ± 0.1%.

Cell voltage ~5V, read to 0.01V

Calorimeter output from strip chart,
18.12 mW/div, 100 div. full scale

CELL
1 x 3.9 x 3.5 cm^3 stainless steel
Recently Ni and Teflon - coated Ni
7.5 - 8.5 ml cell volume.
1.7 ml / 2 day replenishment
Teflon coated leads in head space.

EXPERIMENTAL APPROACH

* MATERIAL

Pd: (i) 0.5mm diameter x 10mm long
(ii) 1.0mm diameter x 10mm long
(iii) 2 mm diameter sphere

Pt: (i) 0.5mm diameter x 10mm long
(ii) 1.0mm diameter coil

*ELECTROLYTE

(i) 0.1M LiOD, 0.1M LiOH, 0.1M NaOD, 1.0M LiOD, 0.,1M ^{6}LiOD, and 0.1M ^{7}LiOD

(ii) Volume: 7.5 - 8.0 ml.

*The rate of heat generation was measured in a closed stainless steel (1cm x 3.9cm x 3.5m). The rate of heat generated by oxidation of the stainless steel in the electrolyte is negligible.

Pd / 0.1M LiOH

[1.48V for ΔH]

0.5 mm d x 10 mm charged at bench 2 days.
Run begun noon 5/26/89.

5/29 (7 hr after 1.7ml replenishment)
1.7 + 6.1 ml

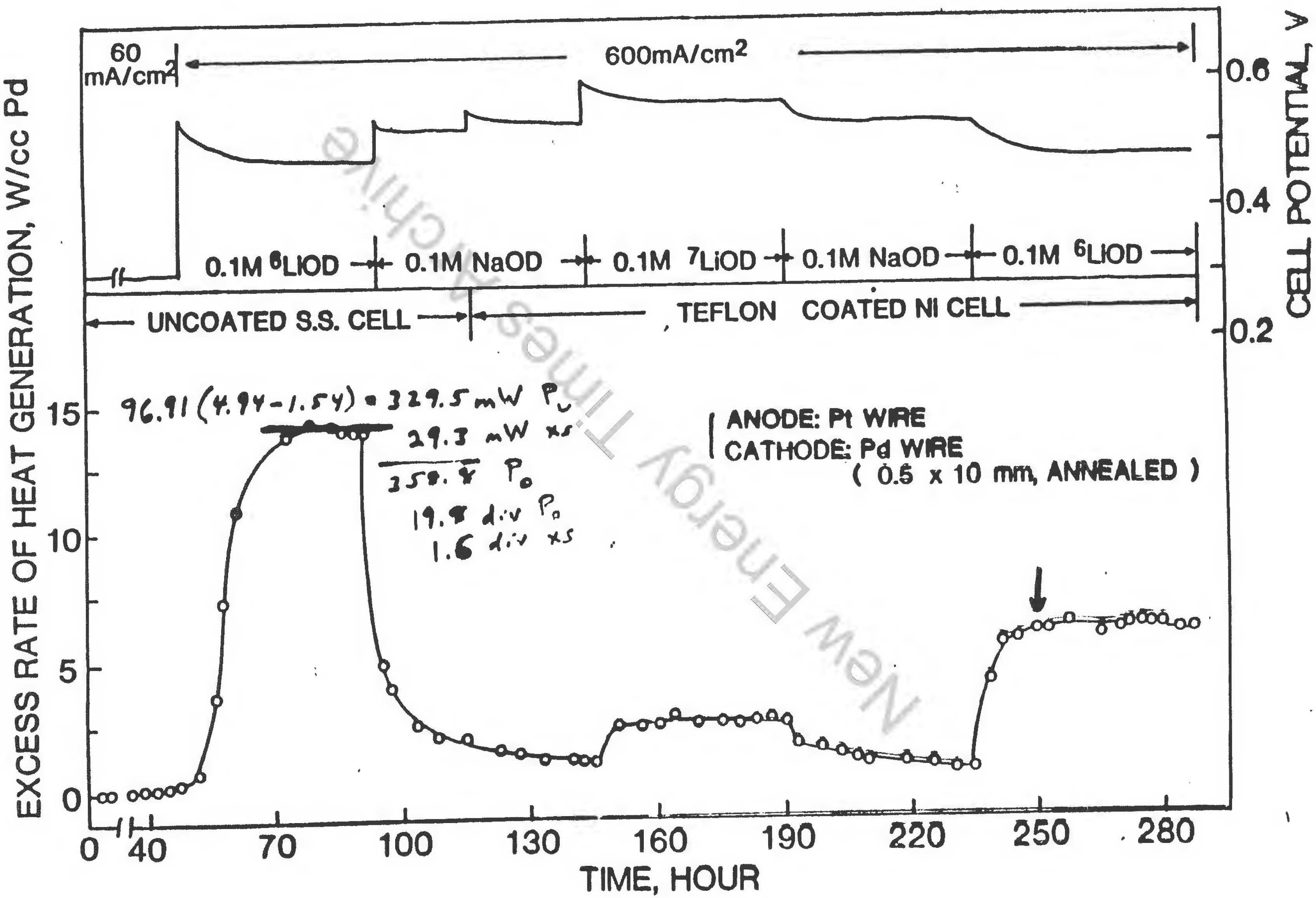
Time	i,mA	V	V-1.48	P_{in} (mW)	P_{out} (mW)	chart div
3:00pm	96.56	4.525	3.045	293.83	291.73	16.1
5:00pm	96.45	4.520	3.04	293.20	291.73	16.1
10:00pm	96.60	4.50	3.02	291.73	289.92	16.0

Pd / 0.1M ^{6}LiOD

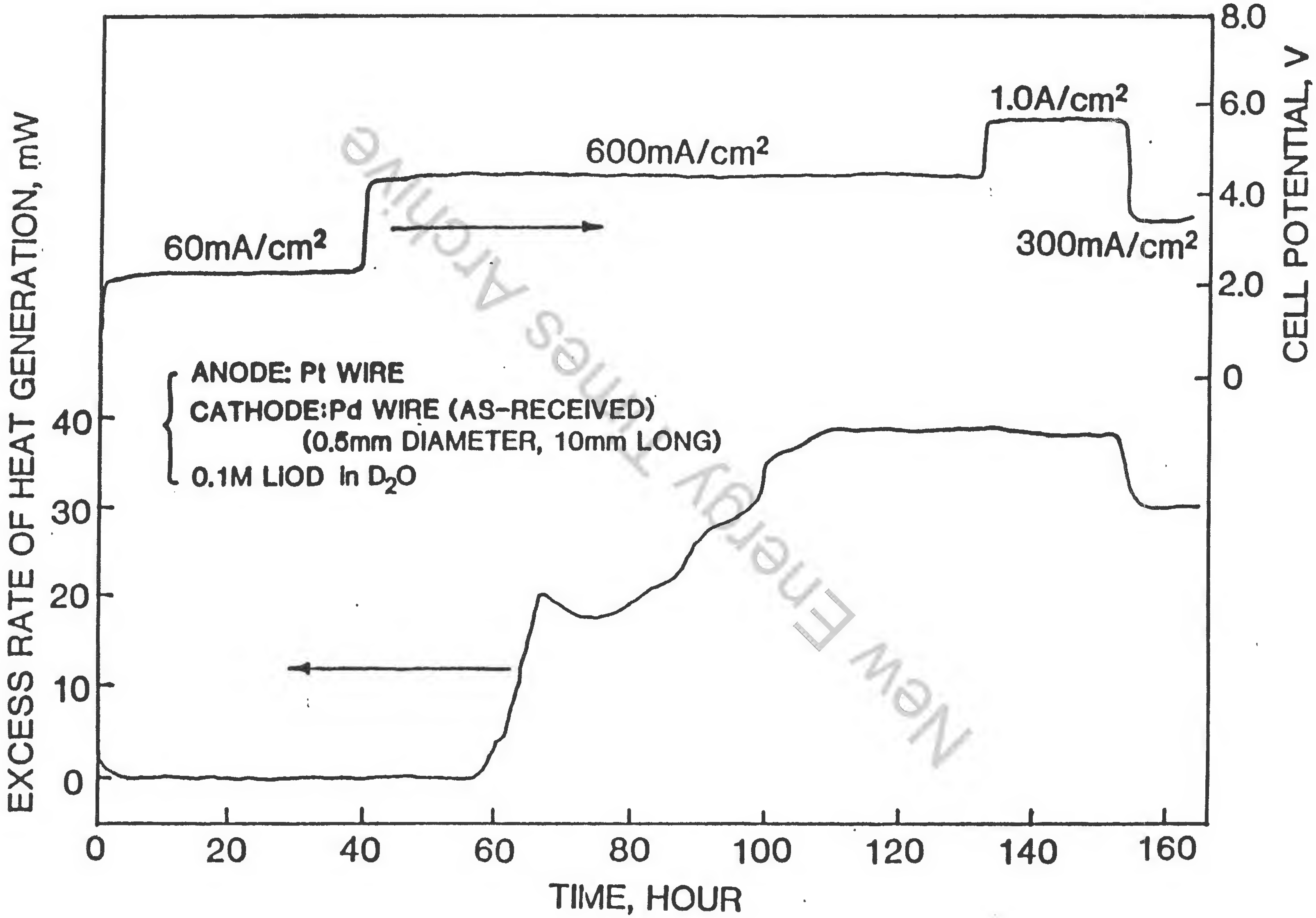
Ni - Teflon

6/12/89

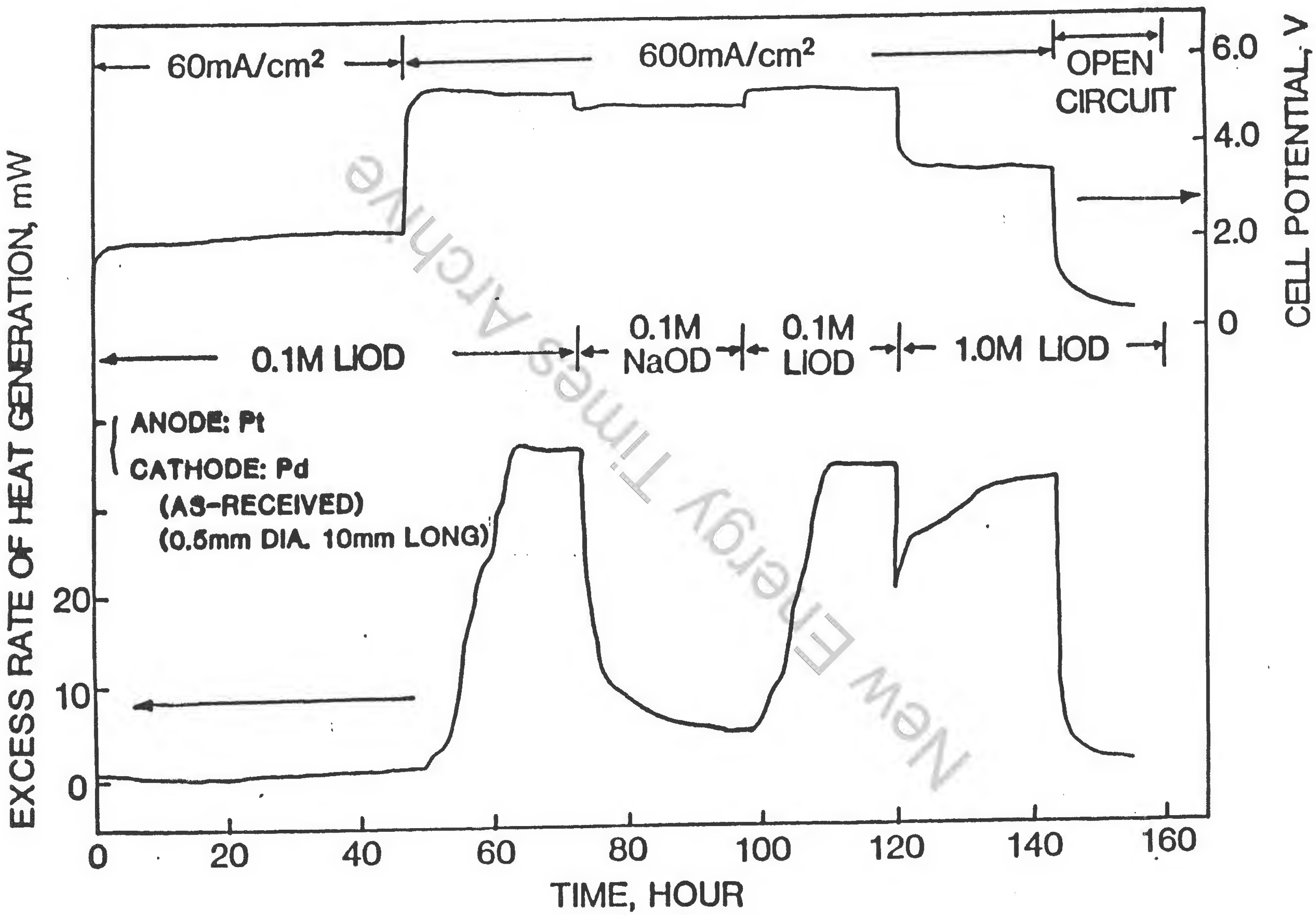
Time	i, mA	V	V-1.54	P_{in} (mW)	P_{out} (mW)	mW gain	W/cm^3
10:15 am	97.00	5.03	3.49	338.53	347.90 19.2 div	9.37 0.5_2 div	4.77



60 mA/cm2
600mA/cm2
0.1M 6LiOD
0.1M NaOD
0.1M 7LiOD
0.1M NaOD
0.1M 6LiOD
UNCOATED S.S. CELL
TEFLON COATED Ni CELL
96.91 (4.94 - 1.54) = 329.5 mW P
29.3 mW xs
358.8 P
19.8 div P
1.6 div xs
ANODE: Pt WIRE
CATHODE: Pd WIRE
(0.5 x 10 mm, ANNEALED)
EXCESS RATE OF HEAT GENERATION, W/cc Pd
CELL POTENTIAL, V
TIME, HOUR



60mA/cm²
600mA/cm²
1.0A/cm²
300mA/cm²
ANODE: Pt WIRE
CATHODE:Pd WIRE (AS-RECEIVED)
(0.5mm DIAMETER, 10mm LONG)
0.1M LIOD In D₂O
EXCESS RATE OF HEAT GENERATION, mW
CELL POTENTIAL, V
TIME, HOUR



60mA/cm²
600mA/cm²
OPEN CIRCUIT
0.1M LIOD
0.1M NaOD
0.1M LIOD
1.0M LIOD
ANODE: Pt
CATHODE: Pd
(AS-RECEIVED)
(0.5mm DIA. 10mm LONG)
EXCESS RATE OF HEAT GENERATION, mW
0
10
20
CELL POTENTIAL, V
0
2.0
4.0
6.0
TIME, HOUR
0
20
40
60
80
100
120
140
160

TABLE: Evidence for "Anamolous Heat Generation" During Electrolysis of D2O in LiOD on Palladium Cathode - Results of Test and Control Experiments

Exp. #	Electrode Material		Electrolyte	Current Density	Rate of Excess Heat Generation
	Cathode	Anode		mA/cm*2	W/cm*3 of Pd
1	Pd 0.5mm dia. 10mm long	Pt	0.1M LiOD	300	16.3
				600	19.3
				1000	18.5
2	Pd 0.5mm dia. 10mm long	Pt	0.1M LiOH	600	0
3	Pt 0.5mm dia. 10mm long	Pt	0.1M LiOD	600	0
4	Pd 1.0mm dia. 10mm long	Pt	0.1M LiOD	600	4 - 7
5	Pd 2.0mm dia. sphere	Pt	0.1M LiOD	600	6 - 12

2. Martin

CELL

Glycerol and air jackets.
Overhead stirrer.
Quartz or thermistor thermometers.
Cartridge heater - tube with glycerol.
1 mm diam, Pd 99.997% Puratronic grade.

HEAT SUBSTITUTION

1. Electrochemistry circuit off or trickle charge.
2. Pass known power P_r through resistor to get heat rise ΔT.
3. Initiate electrolytic current, lower P_r to keep ΔT const. (ΔP_r)

- If no xs cell heat, $\Delta P_r = i(V - 1.53)$.
- If xs power is observed, $\Delta P_r > i(V - 1.53)$.

$$\%xs = \frac{\Delta P_r - i(V - 1.53)}{i(V - 1.53)} \times 100$$

15 Electrodes

One electrode shows xs heat of about 50% at 50 mA/cm^2.

Second compromised by nickel shell of quartz thermometer.

Various pretreatments of Pd

Annealing — 1100°C vac.
300°C Ar
C_2H_2 flame

Recasting in Al_2O_3

Nothing: 1/5

Future

J-M rods: 2, 4, 6 mm on loan.

U. British Columbia calorimeter

3. Bockris

1. Calibrate ΔT - P_r for resistor heater.
   Magnetic stirring.

2. Determine ΔT - P_{in} for Pd cell.
   Correct P_{in} by subtracting 1.54 i

3. If no xs heat, corrected curve and calibration curve should superimpose.
   If not, offset at same ΔT is power due to xs heat

Assumes gas bubbling introduces no further perturbation in heat transfer and that the calibration is not changed over a substantial period of time.

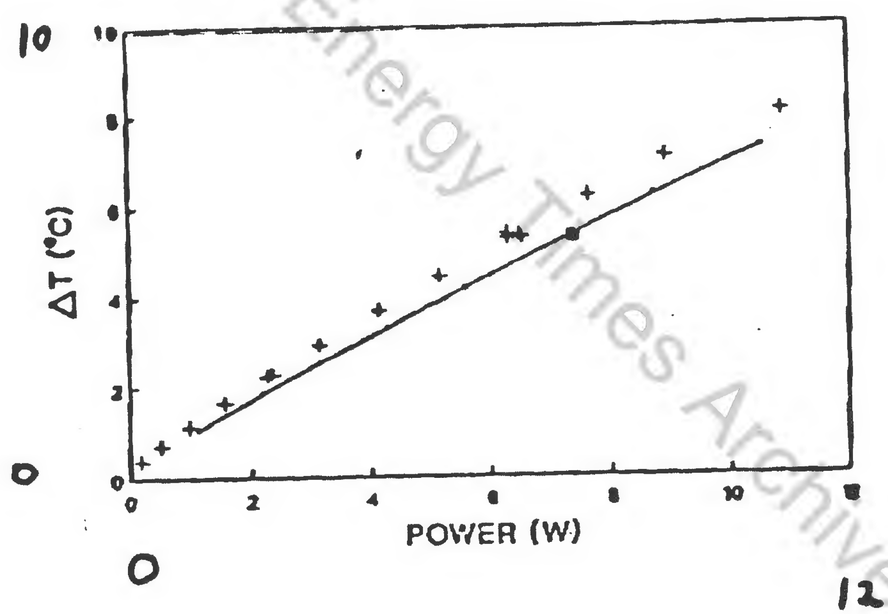
TABLE 1

Pre-treatments and charging solution composition for different Pd electrodes (Sample B 0.3 cm diameter; Sample A 0.1 cm diameter)

Sample	Pre-treatment	Charging Solution	Excess Heat
B1	No treatment	0.1M LiOD	No
B2	No treatment	0.1M LiOD + 0.001M NaCN	No
B3	Annealed at 800°C for 6 hrs	0.1M LiOD	No
B4	Annealed at 800°C for 6 hrs	0.1M LiOD + 0.001M NaCN	No
B5	Acid etch	0.1M LiOD	No
B6	Acid etch	0.1M LiOD + 0.001M NaCN	No
B7	Electrochemical oxide removal	0.1M LiOD	No
B8	Electrochemical oxide removal	0.1M LiOD + 0.001M NaCN	Yes
B9	No treatment, charged in U tube	0.1M LiOD	Yes
A9	No treatment, charged in U tube	0.1M LiOD	Yes

Charged against Ni anodes

^3H test solutions



Electrode B8

TABLE 2

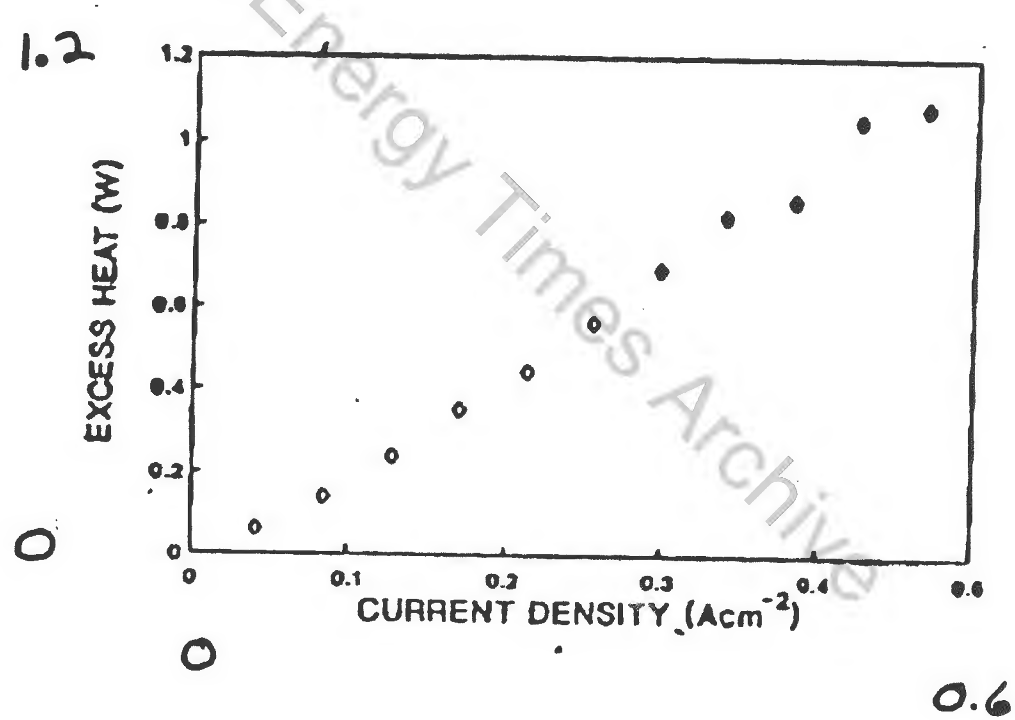
Heat output from sample B8

I [mA/cm^2]	W_{est} (watts)	W_{exp} (watts)	$W_{exp}-W_{est}$ (watts)	$\Delta W/W_{est}$ [%]	
468	8.91	9.99	1.08	12	~1.5°C
425	7.61	8.66	1.05	14	
383	6.40	7.35	0.86	13	
340	5.14	5.96	0.82	16	
298	4.13	4.82	0.69	18	
255	3.15	3.71	0.56	18	
213	2.28	2.72	0.44	19	
170	1.57	1.92	0.35	22	~0.5°C
128	0.98	1.22	0.24	25	
85	0.52	0.66	0.14	27	
42	0.18	0.24	0.06	33	

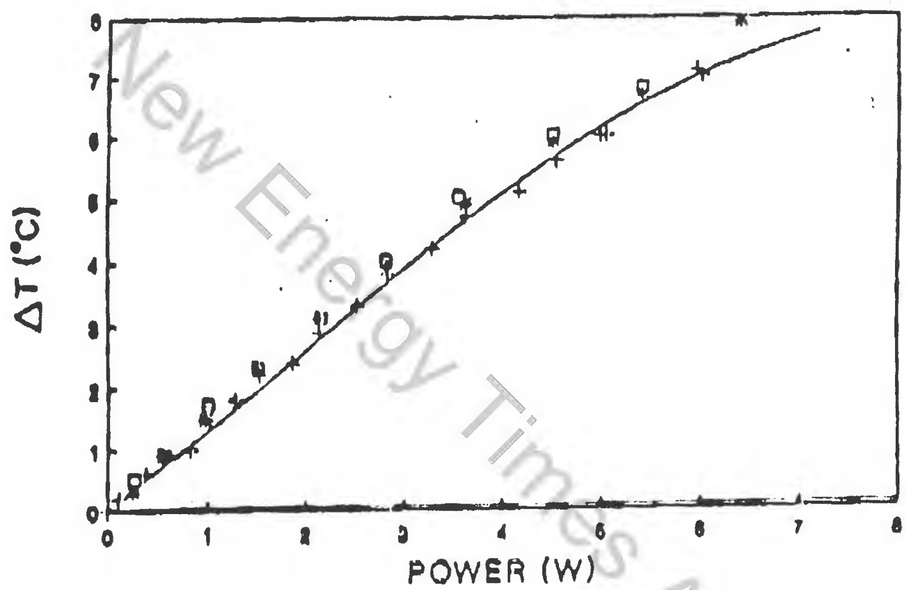
$W_{est} = (E - 1.54)\, I$

~0.7°C/W

Claim ± 0.05 W



B8


ΔT (°C)
0
1
2
3
4
5
6
7
8
POWER (W)

Bockris Group

1. Excess heat in 3 out of 10 electrodes
2. Linear relation to current density
3. 5 W/cm^3 at 400 mA/cm^2
4. Suggest surface rather than bulk effects (dendrites, Ni contamination)

While many scientists have poured cold water on cold fusion, at least one large company is backing further research, as **Clive Cookson** discovered in an interview with Fleischmann and Pons

GE backing softens blow of scientific scepticism

FINANCIAL TIMES FRIDAY JUNE 30 1989

General Electric, one of the largest US companies, is to collaborate with the University of Utah to develop the :old" nuclear fusion discovery of Mar- n Fleischmann and Stanley Pons.

The GE agreement has prompted the overnor of Utah, Norman Bangerter,) ask for the release of $5m of state ınds to set up a fusion research centre t the university. He regards the con- :oversial discovery, first announced on larch 23, as now "confirmed".

This is despite the fact that many boratories have failed to confirm the leischmann-Pons finding that a palla- ium electrode immersed in heavy ater produces large amounts of heat rough nuclear fusion. The UK Atomic nergy Authority's Harwell laboratory ave up its cold fusion research pro- amme after three months of intensive ork by 10 scientists had failed to pro- ce any positive results. Publicity ven to the failure at Harwell has used many people to conclude that ld fusion is dead.

Elsewhere, however, signs of nuclear sion in palladium have been reported. few scientists have measured excess eat output and some have detected the :pected products of fusion: neutrons ub-atomic particles) and tritium (a dioactive isotope of hydrogen).

US institutions where scientists have und some evidence to support Fleisch- ann and Pons include Stanford Uni- rsity, Texas A&M University and Los lamos National Laboratory. "Alto- ther 20 to 30 research groups have ecemeal confirmation of our find- gs," Pons says.

The level of scientific support, com- ned with GE's industrial collabora- on, may be sufficient to persuade the ate to fund a cold fusion research ntre at the University of Utah, despite tense criticism of Fleischmann and ns from many other quarters. The an is for about 40 scientists to work in 22,000 sq ft laboratory on the univer- y research park.

The university expects shortly to sign second industrial collaboration agree- ent – with Johnson Matthey of the (, the world's leading supplier of plat- um metals, including palladium. ohnson Matthey is number one in atinum metal chemistry," says eischmann.

The company is working with eischmann and Pons to identify the tical features of the palladium elec- odes. The patchy experimental results o far suggest that cold fusion is highly ependent on the precise crystal struc- ıre of the palladium and on the level f impurities in it.

"We will continue to do whatever work we consider appropriate to increase our understanding of this interesting but confusing field," says Bruce Tofield, director of the Johnson Matthey Technology Centre.

The advantages of collaborating with GE, one of the world's largest high technology companies, are clear. "Right at the beginning I said we had to work with GE because I wanted to have their patent team on our side," Fleischmann says. "We know their electro-chemists and the overall strength of their industrial laboratory."

GE will initially assign four scientists to work with the university on cold fusion. The company will also make available materials, equipment and other resources. "If we have to design a new piece of equipment with elaborate pipework, they will put an engineer on to it – which will save us having to go to the textbooks to do it ourselves," Fleischmann says.

The two electro-chemists have spent the last fortnight in England. They are secluded in Fleischmann's family home in rural Wiltshire, far from the fusion fever that still grips Utah, trying to rest and to prepare a second scientific paper which will give a fuller account of their solid-state fusion experiments. (They prefer the phrase solid-state fusion because the nuclear reaction takes place in a crystal lattice, rather than in the gas plasma of a conventional fusion reaction.)

They admit that their first paper, published in April in the Journal of Electro-Analytical Chemistry, was not as complete as they would have liked. They are now carrying out more control experiments, with cells containing light water as well as heavy water and a larger number of different electrodes. The input and output of heat is being analysed via two independent calorimetric techniques.

Meanwhile the experiments bubble on in Pons's chemistry lab in Salt Lake City, tended by colleagues who fax the latest readings from the instruments to England every day. "By the end of this summer, we want to get to the position we had hoped to reach by the summer of 1990," he says.

Their plans have been hastened by the early publication, which Fleischmann says was forced on them. This was because a rival research team at Brigham Young University, headed by Steven Jones, wanted to publish the results of similar cold fusion experiments which produced neutrons but no heat. "Knowing as we did that there was an excess release of heat, it was inconceivable that we should not inform the University of Utah and inconceivable that the university should not apply for a patent."

Pons and Fleischmann personally opposed the university's decision to announce their results through the March 23 press conference – an event that brought them not only instant celebrity but also instant hostility from many scientists who thought that they should have followed the traditional procedure of publishing in a scientific journal.

Fleischmann says that he does not understand why the Harwell experiments, which he helped to set up, failed to produce evidence of solid-state fusion. He claims, however, that experiments failed in some other laboratories because "incompetent" physicists did not understand the principles of electrochemistry.

And he denies the suggestion that he and Pons may deliberately have withheld key information about the process in order to delay other scientists while they pursued their experiments. "We have not consciously kept anything back, although we are conscious that there are factors which are important and which people ignore."

Fleischmann now believes that two separate nuclear processes can take place in palladium electrodes. One is a "background process" which produces fairly steady low levels of heat. The most likely candidate for this is two deuterons (deuterium nuclei) fusing to form helium-4; the energy of the reaction is transferred directly to the crystal lattice as heat.

The second process is far more energetic. It gives occasional bursts of heat, tritium, neutrons and radiation. In this :ase, the fusion of two deuterons pro- luces either tritium and hydrogen or ıelium-3 and a neutron. The special :onditions that initiate and terminate he fusion bursts remain unclear.

On their plans for the future, Pons ınd Fleischmann say that they "do not vant to manage a huge solid-state usion development project. However, ve will be involved in developing a cell vhich gives a ready demonstration of :xcess heat generation."

Looking several years ahead, Pons ees the first practical application of olid-state fusion in "providing low- rade heat for space heating.

The next phase would be to use the eat from fusion cells to raise low-pres- ure steam for electricity generation. hey believe that one palladium rod ɔuld eventually produce "a few tens of ilowatts" of energy – enough for a ngle household.